Do the cerebellum and the oblongata represent two encephalic segments or only one? : abstract / by Burt G. Wilder.

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DO THE CEREBELLUM AND THE OBLONGATA REPRESENT TWO ENCEPHALIC SEGMENTS OR ONLY ONE?

By Prof. BURT G. WILDER, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

SINCE the publication of Von Baer's "Entwickelungsgeschichte" in 1837, the segmental constitution of the brain has been more or less distinctly admitted by most writers who have treated the organ in the light of embryology and comparative anatomy.

Under various technical and vernacular designations, the following have recognized two segments between the myel[1] and the

[1] In the paper as presented the common forms *myelon* and *encephalon* were used. In correcting the proof I have substituted for them the English paronyms, in accordance with considerations presented in New York Medical Journal, March 28, 1885, and in the presidential address, "Paronymy versus Heteronymy," before the American Neurological Association, June 18, 1885.

mesencephal (optic lobes, etc.): Harrison Allen, Gegenbaur, Huxley, Kölliker, Lankester, Meynert, Mihalkovics, the editors of Quain's Anatomy (eighth and ninth editions), Reichert, Schwalbe, Wiedersheim and the present writer.

Omitting lesser parts, the cephalad of these two segments is held to include the cerebellum, together with the portion of the "brain stem," immediately connected therewith, and the latter, the part commonly called "medulla oblongata." By Von Baer and most other German writers the former is termed *hinterhirn*, and the latter *nachhirn*. I have adopted the Latin terms employed in Quain's anatomy, viz., *epencephal* and *metencephal*, abbreviated to *epen.* and *meten.* So far as I know, the only original investigators of the subject who have admitted but a single segment caudad of the mesen., are Balfour, Milnes Marshall, Owen and Spitzka, and the last named alone has discussed the question at any length.

That the entire region caudad of the mesen. should be regarded as a single segment, of which the cerebellum is "merely a dorsal hypertrophy" has been urged by Spitzka in 1878 ("Jour. of Nerv. and Mental Disease") in 1881, ("Science," April 9), and in a recent communication to "The New York Medical Record," July 26, 1884.

Before discussing the main question, I desire to correct a misapprehension into which Spitzka seems to have fallen respecting my own opinions. He implies, first, that I am committed to the view commonly entertained, and, second, that I regard the *pons* as determining the cephalocaudal extent of the epencephal.

On the contrary, as stated in "The Medical Record," Aug. 9, 1884, I have repeatedly expressed my sense of the difficulty of determining the precise number and limits of the encephalic segments, and admitted the force of Spitzka's own view. As to the pons, when present it strengthens the floor of the segment, and serves as a landmark of that region; but it by no means follows, and has never been implied by me, that either of its margins, especially in man, indicates the limits of the epencephalic segment, although in the cat and other mammalia with moderately developed cerebellums, one or both of its margins may closely correspond therewith.

In his able presentation of objections to the bisegmental con-

stitution of the region in question, Spitzka overlooks what appear to me to be weighty arguments in favor of the prevalent view, viz.:

(1) The existence, in the embryo, of a constriction of the "primary hind-brain" by which the "secondary hind-brain" is distinguishable from the "after-brain."

(2) The demarcation, at least in man, of the cephalad (epencephalic) portion of the "fourth ventricle," from the caudad (metencephalic) portion, by the transverse fasciculi called *striæ acusticæ*.

(3) The presence, in several Teleosts and Elasmobranchiates, of dorso-lateral, metencephalic masses, united by a thinner (commissural?) portion, and connected with the cerebellum only through a thin or membranous lamina comparable with the *valvula* between the cerebellum and the optic lobes. In the Torpedo (as seen in the preparation and photograph exhibited) each of these metencephalic masses exceeds in size the dorsal portions of all of the other segments together.

(4) In addition to these direct arguments, is the indirect one that most of the objections to the recognition of a cerebellar segment apply with equal force to the universally accepted mesen., and with even greater force to the oblongata (and thus to the brain as a whole) in relation to the myel. In other words, the meten. is more sharply defined from the epen. than it — and thus the entire brain—is from the myel.

(5) Finally, so far from the metepencephalic region embracing but a single segment, even if we hesitate to adopt Clevenger's idea that the cerebellum represents a series of coalesced intervertebral ganglia, we cannot ignore the suggestion of Balfour, based upon the observations of Gegenbaur, Marshall and himself, that several segments, each coördinate with the mesen., are represented in the region between it and the myel.

In view of all these considerations, it seems to me, first, that further investigations in embryology and comparative anatomy are needed to place upon a satisfactory basis any segmental formula of the brain; and, second, that the objections advanced by Spitzka should not prevent our entertaining and employing, as a convenient "working hypothesis" the interpretation made by Von Baer, and accepted by the majority of later authorities upon the subject.

The existence and dorsal circumscription of the porta (foramen of Monro) in the adult human brain. By Prof. Burt G. Wilder, Cornell University, Ithaca, N. Y.

[ABSTRACT].

In a paper, "On the foramina of Monro in man and the domestic cat," presented to the Association at the Boston meeting in 1880, it was stated (1) that no recent writer had denied the existence of a communication between each procœle (lateral ventricle) and the aula (cephalad part of the "third ventricle"); (2) that I had not yet had the opportunity of examining the parts upon a properly prepared human brain, but the imperfect materials at my disposal indicated that the relations are essentially the same as in the cat.

Since then, however (1) I find that, at the "Société de Biologie," May 14 and June 21, 1879, Prof. Mathias Duval declared that "when one finds a communication between the middle and lateral ventricles in an adult human brain, it results from an artificial perforation;" (2) I have been able to determine the existence and dorsal boundary of the porta in all the human brains examined for the purpose, including several adult as well as fetal and young. Some of these, like the specimen exhibited, were prepared by the process of continuous, cœlian alinjection (alcoholic injection) described by me at the last meeting of "The Society of Naturalists of the Eastern United States," and in "The New York Medical Journal," March 1, 1884. The photographs (taken by Prof. S. H. Gage) here shown are believed to be the first ever made of "the foramen of Monro;" in the enlarged one the orifice is seen to be bounded caudad by the thalamus, cephalad by the fornicolumn, ventrad by the continuity of the two, and dorsad by the endyma reflected from the parts named upon the intruded portiplexus.

As has been repeatedly stated by me, this demonstration of the dorsal limit of the porta is equally a demonstration of the ventral limit of the rima ("great transverse fissure") with which the porta is usually, but erroneously, made continuous in both figures and descriptions.[1]

[1] Since this paper was presented, I have been enabled to see the same points to even better advantage upon an alinjected hydrencephal (hydrocephalous brain), shown at the meeting of the Am. Neurological Assoc., June 18, 1885.

The relative position of the cerebrum and the cerebellum in anthropoid apes.

By Prof. Burt G. Wilder, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

So far as I am aware, the discussion as to whether or not the cerebellum of the anthropoid apes is completely covered by the cerebrum has not been based upon the examination of a brain hardened within the cranium, or upon photographs taken before its removal. Through the courtesy of Prof. H. C. Chapman, M. D., one of the directors of the Philadelphia Zoölogical Gardens, and of Mr. Arthur E. Brown, superintendent of the same, the opportunity was afforded me, last October, of so treating the brain of a young female chimpanzee, supposed by Mr. Brown to be about four years old; the day after its death it weighed 6.7 kilograms.

For ten days, at a temperature ranging from four to fifteen degrees centigrade, the entire animal was subjected to the process of *continuous arterial alinjection* (alcoholic injection) described by me at the last meeting of the Society of Naturalists of the Eastern United States, and in "The New York Medical Journal," for Feb. 23, 1884, and elsewhere. On the second day, less than half of the calvarium was removed, and photographs taken of the dura-covered brain; later, after the removal of the dura, and then of the other part of the calvarium, and of the brain itself, photographs were taken from several points of view.

As shown by these photographs and by the left half of the brain, here exhibited, when the specimen is held so that the ventral outline of the orbital lobe is horizontal, the most prominent portion of the cerebellum is overlapped at least 1 mm. by the extremity of the occipital lobes of the cerebrum.

Admitting the possibility, suggested by Chapman, of individual; age and specific variations, it is probable that, in some cases, as suggested by Spitzka, the conclusion that the anthropoid cerebellum is not overlapped by the cerebrum has been due to the distortion of the brain during or after its removal.

In addition to these considerations, it is evident that the relative positions of the parts will depend upon the way the brain is held. Until evidence is given to the contrary, I assume that the brain should be held as if the animal were looking horizontally.

SOME QUESTIONS IN ANATOMICAL NOMENCLATURE. By Prof. BURT G. WILDER, Cornell University, Ithaca, N. Y.

[ABSTRACT.]

SINCE 1870, in various publications, I have urged upon anatomists certain modifications of the existing terminology of description and designation. Even by those who have not accepted my specific recommendations, it is quite generally admitted that some change is needed. I now ask for the appointment of a "Committee on Anatomical Nomenclature," by Section F, to report at the next meeting upon the general subject, especially with respect to the brain; this committee to invite the coöperation of anatomists of other nationalities, and to confer respecting encephalic nomenclature with the Committee on that subject appointed at its last meeting by the American Neurological Association.

The following are examples of the more general and special questions to be considered by such committee:

1. Is it desirable to employ the words *toponymy* and *organonymy*, respectively, for the subjects of descriptive and designatory terms?

2. Is it practicable, at present, to harmonize the toponymic terminology of vertebrates with that of any invertebrates?

3. Should we not agree upon a certain normal position of the vertebrate body, including the limbs, and endeavor to use toponymic terms applicable equally to all?

4. Should not *postaxial*, *epaxial*, etc., as applied to the limbs, be replaced by terms referring to the regions of the entire body.

5. Shall the median plane or line be designated by those or other phrases, by Barclay's *mesium*, or my *meson* (τό μέσον, the middle), with the derivatives *mesal*, *mesad* and *meso?*

6. In designating relative position along the longitudinal axis of the body, since *oral*, *cephalic* and *caudal* are commonly employed in an absolute sense, shall we follow the analogy of *northward* and use the adverbial adjectives *orad*, *cephalad* and *caudad;* or new terms like *prorsal* and *retral;* or the familiar words *anterior* and *posterior*, with the understanding that they are never to be used in the anthropotomic sense of *ventral* and *dorsal?*

7. Should not organonymic terms (terms of designation) be,

as far as practicable, *brief; capable of inflection; classic in derivation and form*;[1] *already used in a kindred sense?*

8. Since the length of a term may depend upon not only the number of syllables and letters, but also upon the number of separate words, and since, properly, only single words are capable of inflection, is there not a twofold reason why the names of parts, with certain self-evident exceptions (nerves, etc.) should be *mononymic?*

9. Is there much real analogy between the nomenclature of anatomy and that of zoology and botany?

10. How far should priority be regarded in the selection of existing names?

11. Can priority be claimed for terms which are vernacular or descriptive?

12. In considering all questions of terminological reform, should we not regard less our present and personal convenience, than the interests of the vastly more numerous anatomical workers of the future?

[1] In later papers ("Encephalic Nomenclature," N. Y. Med. Jour., April 21, 28, 1885, and "Paronymy versus Heteronymy as Neuronymic Principles", the presidential address before the American Neurological Association, 1885, "Jour. of Nervous and Mental Disease," July, 1885), I have suggested that, in English works, so far as possible, the *names be given an English aspect by paronymisation.* For example, *commissura* becomes *commissure; pedunculus, peduncle; mesocœlia, mesocœle; myelum, myel; and encephalum, encephal.* With many names no change is needed, as with *porta, aula, fornix, callosum,* etc. *Heteronyms* or vernacular translations are regarded as objectionable. The same principle is applicable to other languages.

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