

Dr. David W. Day, Mr. William Glenn, of Baltimore, and others.

In response to the formal motion made and carried, the president appointed a committee consisting of Professor Munroe, Dr. Clarke and Dr. Wiley to draft resolutions expressing the loss felt by the Washington Chemical Society in the death of Dr. Bolton.

A. SEIDELL, *Secretary*.

SHORTER ARTICLES.

SOME OSTEOLOGICAL TERMS.

IN the usual osteological nomenclature, there are certain terms, among others, which have been and yet are so loosely and indefinitely used that one is often in doubt as to their meaning. I refer more especially to 'hæmapophysis,' 'hæmal spine' and 'hypapophysis.' The first two of these were proposed by Owen in the *Geological Transactions*, Vol. V., p. 118 (1838). 'Hæmapophyses' was there defined and used as a synonym of 'chevron-bones'—"These are the chevron-bones of Mr. Conybeare, the paravertebral elements of Geoffroy St. Hilaire." In later years, especially in his 'Archetype and Homologies of the Vertebrate Skeleton,' Owen extended the meaning of the word to include the ischium, pubis, costal cartilages, etc., and he correctly suggested it for the intercentrum of the atlas in 1851. Cope in his posthumous work upon the lizards and snakes of North America uses hæmapophysis as a synonym of rib. As applied to the chevron-bones, the word is unnecessary, and, as extended to the other structures in Owen's transcendental theory, the term is inapplicable and mischievous. As is well known, the 'hæmapophyses' of fishes are formed chiefly by the deflection of the parapophyses, while the chevrons of reptiles are supposed to be of intercentral origin alone. Unfortunately, the phrase 'hæmal arch' has also had a very indefinite application, but its use is preferable to that of 'hæmapophyses.' In any event, I quite agree with Boulenger that the latter word should be banished utterly from anatomical nomenclature. The word chevron has become well fixed, and has, moreover, the advantage of being morphologically meaningless.

'Hæmal spine' was first proposed by Owen to indicate the spine of the united chevron. In this application among fishes it has a definite morphological meaning, though not often now so used. The term helped Owen to round out his symmetrical archetype of the vertebra, but, when he later applied it to so incongruous an assemblage of morphological elements as the sternum, episternum and hyoid, as well as the intercentra of the Squamata, it loses every particle of meaning it may have once had and should be discarded. Boulenger, however (*Proc. Zool. Soc. Lond.*, 1891), has proposed to use the phrase in a totally different sense from any suggested by Owen for the infracentral keel or spine of such vertebræ as those of the turtles, rabbits, etc.

Concerning 'hypapophyses' there is ground for differences of usage, yet I think it may be shown that the word should be restricted to those processes only which Boulenger would call hæmal spines. The term was not proposed by Owen until some time after he had formulated his archetypal theory, appearing, I think, for the first time in his 'Skeleton and the Teeth,' published in 1853 or 1854, where it was defined. It seems clear from this definition, as also from his discussion of the vertebra in his 'Archetype and Homologies,' that he intended the word primarily for infracentral exogenous processes. He calls the hypapophysis exogenous, but says it may sometimes be autogenous, like 'the diapophysis and the parapophysis.' As we now restrict the latter two terms solely to exogenous processes, the former should be also. Boulenger, however, prefers to apply the term to the autogenous elements alone, that is to the intercentra and chevrons, and so uses the word as a synonym of 'intercentrum.' Baur, apparently following Boulenger, in 1894 (*Proc. Nat. Mus.*) invented the term 'catapophysis' for what was evidently originally meant by hypapophysis, and what is called hæmal spine by Boulenger, and accepted hypapophysis in place of intercentrum.

Cope was the first to use the term intercentrum in the sense now employed for the hypaxial element in the amphibia and reptiles. The element in question, however,

had previously been called *Zwischenwirbelbein* by Von Meyer in *Sphenosaurus*, and, long before, Egerton, in 1836, had proposed the phrase 'subvertebral wedge-bone' for the same element in the ichthyosaurs. It may be of interest to observe that Marsh, as early as 1878 (*Amer. Journ. Sci.*, May), correctly recognized his 'intercentral bones' in the so-called hypapophyses of the Mosasaurs, though Boulenger, as late as 1891, denied their identity. Hypapophysis is yet frequently used for the intercentrum of the atlas, following Owen, and 'hypocentrum,' 'basiventral bone,' etc., are frequent and superfluous synonyms of intercentrum.

There is yet another anatomical term which bids fair to become confused in its application—splenial. Owen proposed the term ('Arche-type and Homologies,' p. 15) in place of the Cuvierian 'opercular,' a term inadmissible because of its double use in the fishes, for the splint-like element on the inner side of the mandible, and figured as typical of the mandible in the crocodile and ostrich. Baur, correctly, I believe, recognizing that the so-called splenial of the turtle is not morphologically identical with the splenial in the crocodile and lizard, but rather a dermal element separated from the articular, gave to it (improperly, I think) the name of angular, while the real angular he called the splenial, and for the real splenial he proposed the new name 'presplenial.' Lambe, recently, in his description of the mandibular elements in *Dryptosaurus*, retains the names previously used in the turtles, but calls the most anterior element, sometimes also present in the turtles, the presplenial. But, this is inadmissible. There can be little if any doubt but that the presplenial of *Dryptosaurus* and the testudinates is morphologically identical with the real splenial of the crocodiles and the lizards, and it must receive the same name. If we call it the presplenial, then Baur's arbitrary change of the angular must also be accepted, otherwise the crocodile, to whose mandible the name splenial was originally applied, is juggled out of a splenial entirely!

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THE ORIGIN OF FEMALE AND WORKER ANTS FROM THE EGGS OF PARTHENOGENETIC WORKERS.

DZIERZON's celebrated theory, according to which the unfertilized eggs of the honey-bee give rise to males, or drones, whereas fertilized eggs develop into females (queens or workers), has not only become one of the established tenets of apiculturists, but has also been expanded by theorists to include other social insects, such as the ants and social wasps. Nor is this expansion merely the result of a tempting analogy. Forel* and Lubbock† long ago showed that the eggs of parthenogenetic worker ants may develop into males, and more recently similar observations have been made by Miss Fielde.‡ These facts certainly confirm the Dzierzon theory and appear to justify its extension to the ants.

The further question, however, as to whether the unfertilized eggs of bees and ants may not, under certain conditions, give rise to workers, is still unanswered.§ In other words, the observation of a number of cases in which males developed from unfertilized eggs, is not in itself sufficient to preclude the possibility of the development of females or workers from such eggs under other circumstances. We know that this possibility is realized in the autumn broods of plant-lice, water-fleas, etc. That it may also be realized in ants is shown by the following observations made independently by three different observers and here quoted as a basis of suggestion for future experimental work. It is, perhaps, timely to stress these observations, for theorizing on sex determination is much in vogue and is being indulged in by some who seem to derive their facts from any but the original sources. That some of these observations have been 'snowed under'—*todtgeschwiegen*, as the Germans say—is not a matter of surprise when we consider the blinding

* 'Les Fourmis de la Suisse,' 1874, pp. 328, 329.

† 'Ants, Bees and Wasps,' London, 1888, pp. 36-40.

‡ 'A Study of an Ant,' *Proceed. Acad. Nat. Sci. Phila.*, July, 1901, p. 439.

§ See also Pérez, 'Mémoire sur le Ponte de L'Abeille Reine et la Théorie de Dzierzon,' *Ann. Sc. Nat.*, 6 ser., Tome VII., Art. 18, 1878, pp. 1-22.