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Function of Claws' claws

SIR—Two possible ecological roles have been proposed by Charig and Milner for the new theropod dinosaur *Baryonyx*[1]. The skull and skeleton of *Baryonyx* show several apparent adaptations to feeding on fish (ichthyophagy) including a long, narrow snout with a terminal expansion, numerous finely serrated teeth and well developed forlimbs and claws. It is suggested that *Baryonyx* crouched over the bank of a river and used its claws to hook fish out of the water rather like a grizzly bear. The alternative lifestyle of a scavenger received very little attention. I suggest that *Baryonyx* could have been a specialized scavenger, which fed almost exclusively on the viscera of large dinosaurs such as the contemporaneous *Iguanodon*.

Crocodilians are the sole surviving archosaurs and they have great difficulty in breaking through the skin of their large mammal prey in order to feed on the flesh. The only way that they can achieve this is by gripping hold of part of the carcase with their jaws and twisting their bodies around violently in order to break into the body cavity[2]. It is possible that large theropods would have faced similar problems in breaking through the skin of their dinosaur prey especially if it were armour plated.

The weakly developed mandible and teeth would make *Baryonyx* particularly unsuited to killing and feeding on large herbivorous dinosaurs but the massively developed forelimbs, with their huge claws, would be ideal for breaking into a carcase. The narrow snout would be well suited to investigating the interior of the body cavity and the flexible hinge between the maxilla and premaxilla may allow much more movement in the restricted space. The slender, finely-serrated teeth would be used in processing the soft viscera. If Baryonyx was a specialized viscera feeder, its long neck and facultative quadrupedalism could be put to good use while feeding on the ground.

Why should this proposal be preferred to that of fish feeding? It seems to be that *Baryonyx* has too many adaptations for fish feeding. Why does it have both forelimbs for hooking fish, like a grizzly bear's, and teeth and jaws similar to the fish-eating gavial's, when one adaptation would suffice? It also seems unlikely, though not impossible, that a creature of 1–2 tonnes[1] could have been sufficiently manouverable to catch fast-moving fish.

On the other hand if *Baryonyx* was a specialized scavenger, as I propose, the need for both the forelimbs, to open up carcases, and the narrow snout, to enter the body cavity, would be explained. In addition the position of the nares far from the tip of the snout would allow *Baryonyx* to feed and breathe at the same time.

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Vagaries of nomenclature and the rigidity of the Code

SIR—Recent criticisms[1-4] of the International Code of Zoological Nomenclature are relevant to a remonstrance I received from some of my entomological friends, for having dared to point out the improper (grammatically) spelling of names of several *Anopheles* species in a book on practical malariology[5].

I am referring here to my deliberate correction of wrong spelling of *Anopheles cruzii, A. bancroftii* and *A. sergentii*, which should not have a second *–i* at their caudal ends. These vectors of malaria were named in honour of Cruz, Bancroft and Sergent (not of Cruzius, Bancroftius and Sergentius), the genitive singulars of which in Latin do not require a double *–i*.

In the third edition of the International Code (1985)[2] one reads (Article 31 para *a*) that "a species group name, if a noun in the genitive case formed directly from a modern personal name is to be formed by adding to the stem of that name *–i*, if the personal name is that of a man ... *–ae* if a woman". Elsewhere (Article 33, d), however, it says that "the use of the termination *–i* in subsequent spelling of a species-group name that is a genitive based upon a personal name in which the correct original spelling terminates with *–ii* constitutes an incorrect subsequent spelling, even if the change in spelling is deliberate".

The result of such edicts is to perpetuate the extraordinary inconsistency in the names of mosquitos, derived from the names of persons. Thus we have *Anopheles bancroftii* Giles and *Culex bancroftii* Theobald, but *Aedes (Aedes) bancrofti* Taylor and *Aedes (Stegomyia) bancrofti* Skuse.

In preparing my book, I have attempted to avoid any ambiguity by using the grammatically correct spelling of names of the 3 most important vectors. I have added a note, that according to the present rules these names should have a different (viz. original) spelling only in a formal citation of the species, including the author and date. But this has not satisfied some purists to whom any infringement of the rules of the Code is tantamount to high treason.

The work carried out periodically by the International Commission on Zoological Nomenclature is admirable in the face of growing difficulties. But the excessive rigidity that prohibits some minor and logical changes of the Code perpetuates errors and inconsistencies for the sake of preserving every vestige of 'stability', even if such stability is illusory[1-4].

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Volumes apart

SIR—Zimmerberg and Parsegian (*Nature* **323**, 36-39; 1986) have introduced a new method to study the pores of ionic channels. They report large changes in the internal volume of a channel protein – the volume inaccessible to polymers – when the channel opens or closes.

The internal volume of a channel protein when defined in this way should not, however, be identified with the volume of the aqueous pore through which ions flow. The channel protein might contain other polymer-inaccessible regions, outside the pore, that change in volume when the channel opens or closes. The total polymer-inaccessible volume change estimated experimentally would then be the sum of several components, only one of which is the volume of the pore. In such a case, the estimated internal volume change might be positive or negative and could exceed the volume of the pore itself.

While I certainly hope such complexities are not widespread in ionic channels of physiological interest, it seems advisable to test the possibility experimentally, perhaps exploiting the pharmacological properties of the pore or differences in accessibility of the pore and other regions of the channel protein to charged solutes.

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PARSEGIAN AND ZIMMERBERG REPLY — We thought the distinction in definition of volumes was obvious in the title and the text. If not, we hope that similarly bemused readers will benefit from Eisenberg's elaboration of this point.

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