

have been produced by protists, fungi, chlorophytes or cyanobacteria, i.e. they may be of animal origin or not. Tubbs denies this situation encountered in daily ichnological work in stating 'if the agent is known not be an animal the Code does not apply'. Following this personal interpretation of Article 1.2.1, an ichnotaxon would be subject to the Code as long as its producer would be unknown; it would fall outside the provisions of the Code as soon as the non-animal origin of the trace fossil could be demonstrated. This argument can be considered unrealistic because the producer can never be identified with certainty. There will always be a chance that extinct or unknown organisms have produced structures that look like those nowadays resulting from different life activities.

Even more dramatic is Tubbs's misconception that 'fossilized works of animals' and 'trace fossils' are synonymous (para. 3). This statement, which is demonstrably erroneous, is a circular argument based on the current wording of the Code. There are many more trace fossils than those of animal origin but neither the botanical nor the bacterial Code contains provisions for ichnotaxa. This means that an ichnotaxon going back to an unknown or non-animal producer currently has no 'legal' standing. For these reasons, we adhere to our opinion that any trace fossils irrespective of their origin should be covered by the zoological Code.

Some other points are uncontested. We understand the reasoning provided by Tubbs not to revoke Article 1.3.6 as originally proposed. Also, ichnologists will gladly accept the clarification that ichnofamilies require typification and that ichnofamilies do not compete with biotaxa.

### Additional references

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**Comment on the neotypification of protists, especially ciliates (Protozoa, Ciliophora)**  
(General Article; see BZN 59: 165–169; 60: 48–49, 143, 216–217)

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I am against the proposal in this general article that Article 75.3.6 should be waived in relation to ciliates, other protists and small Metazoa. Successive editions of the Code have regarded the designation of neotypes as unusual acts, admissible only in exceptional circumstances. Therefore, rules governing the designation of a neotype are numerous and stringent, designed to ensure that a neotype will come as close as possible to the original concept of a nominal species.

Foissner proposes to relax the rules governing the designation of neotypes for Ciliophora and other groups of protists, and possibly even small Metazoa. As far as I am concerned, this proposal is unacceptable because it goes against both the letter and the spirit of the Code. It threatens the universality of the Code by trying to create a special interest group. Very few, if any, taxonomists have the luxury of working with a group that is free of nomenclatural problems. If we were to accept the logic of Foissner's proposal, there is a danger that each generation would be entitled to throw out the types prepared with 'outdated' techniques and allow 'authoritative' redescriptions to be made and supported by new neotypes. Advances in techniques and methods are valuable aids for taxonomy and accompanying nomenclature, they cannot be allowed to steer or control taxonomy and nomenclature. Relaxing the requirements of the neotypification process for ciliates while maintaining these same requirements for all other organisms would destroy the universality of the Code. Furthermore, it carries the very real possibility of students of other groups also making applications to waiver or to relax other requirements of the Code to facilitate their endeavours. That would destroy the universality and the authority of the Code.

**Comment on the proposed precedence of *Ovula gisortia* Passy, 1859 over *Cypraea coombii* J. de C. Sowerby in Dixon, 1850 (Mollusca, Gastropoda)**  
(Case 3220; see BZN 59: 173–175; 60: 218–220)

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I disagree with the point of view of Todd (BZN 60: 218–220) on the following points:

1. The type material of *Cypraea coombii* only contains the complete specimen figured by Sowerby in Dixon (1850) and several remains. The specimen no. 5 mentioned by Schilder corresponds to *Gisortia tuberculosa* (Duclos, 1825) from the Ypresian of the Paris basin. I also stress that Edwards recorded a specimen which was originally deposited in the Museum of Bowerbank, but which was destroyed. This specimen possessed a large callosity on the dorsal face that differed from that of the type of *G. coombii*. This feature makes *G. coombii* closer to *G. gisortiana* than *G. tuberculosa*, as supposed by the authors who had erroneously regarded *G. coombii* as a variety of *G. tuberculosa*.

2. Todd disagreed with the use of the application of Article 81.2.3 of the Code for reasons which are unclear. When he referred to the geographic and stratigraphic



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