

Comments on the proposed conservation of *Stictostroma* Parks, 1936 (Porifera, Stromatoporoidea) and designation of *S. gorriense* Stearn, 1995 as the type species (Case 2901; see BZN 52: 18–20)

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The suggestion that *S. gorriense* be designated as the type species of *Stictostroma* is straightforward and hardly needs discussion. However, I object to the proposal that *Stictostroma* be ruled to be available from Parks (1936). Since Parks did not designate a type species the name was first made available by Galloway & St. Jean (1957), and under Article 13b of the Code they are the authors of the name. Unless there is a synonym published between 1936 and 1957 (and the application does not mention one) the name *Stictostroma* could continue in use in its accustomed sense. The authorship does not form part of a name (Article 51), and the plenary powers should not be used to rule on authorship when there are no other nomenclatural consequences.

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I support Dr Stearn's proposals, which maintain Parks's original concept of *Stictostroma* and its usage since. As stated in para. 5 of the application, Stearn (1995) has shown that the original material of *Stromatopora mammillata* Nicholson, 1873 (renamed *Stictostroma mamilliferum* by Galloway and myself in 1957) lacks diagnostic features. In contrast, the well preserved holotype of *Stictostroma gorriense* Stearn, 1995 was used by Parks (1936) when describing *Stictostroma*, and it has served as the reference (i.e. it has been the de facto type) for the determination of the characteristic internal skeletal morphology and micromorphology of the genus (to which some 40 species, most of them valid, have been assigned). The designation of *S. gorriense* as the type species will maintain the consistent interpretation of *Stictostroma* Parks, 1936.

Comment on the proposed conservation of the specific name of *Xerophila geyeri* Soós, 1926 (Mollusca, Gastropoda)

(Case 2870; see BZN 51: 105–107, 336–338; 52: 176–178)

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I write in reply to Dr Kadolsky's comment (published in BZN 52: 176–178; June 1995). Clearly his main problem concerns the Code, and less so my application. My reply concerns the latter.

1. It appears to be necessary to emphasize once more (see BZN 51: 338) that I did not use what Kadolsky calls 'the opportunity offered by Article 79c ... for sloppy

work and easy glory'. In this particular and in other cases, I really did 'bother to check whether older names, presently regarded as synonyms, are available', studying original descriptions and type specimens. This fact does not fit well into Kadolsky's argument and makes the discussion rather bizarre. Of course, old names should always be used instead of creating new ones. In accordance with the Code, however, unused old names should not replace well-known younger ones.

2. I feel that there is some inconsistency in Kadolsky's attitude to facts and objectivity. In his view my 'subjective' comments 'contribute nothing to the solution of the problem, and should not have any bearing'. Why should that be? Subjectivity cannot be avoided in cases like this. The application is, indeed, subjective and Kadolsky's argument, following an initial incorrect conclusion (see para. 3 below), is not itself an example of objectivity (see para. 4 below).

3. It is incorrect to conclude that 'Gittenberger found 25 citations' for the usage of the name *geyeri* from the fact that I listed such a number, and it is odd that my previous clarification concerning this point (BZN 51: 338) has been neglected as if it were untrue. Is there a formal rule, or even a good reason, according to which we should spend time putting together as long as possible a list of references? Should that list be printed? I can imagine more useful activities and ways to spend money.

4. According to Kadolsky, the species *Trochoidea geyeri* is 'still one of the less frequent of the European land snail fauna'. The reasons for this statement are not given, which raises the question on what authority the notion is based. Opposing his view, I reiterate the fact that the specific name is indeed well known. The criteria for being considered well known are given in the Code. I selected references from various languages and disciplines to demonstrate the usage of the name (see para. 3 above). Additionally I referred for authority to *Zoological Record*, Mollusca (1967 and following years). Kadolsky's statement that 'it is easy to obtain such a number of citations [25] even for less important species' is mischievous because it might be incorrectly assumed that only 25 references were found. Apart from this, the implication that there are other, independent criteria to measure the importance of species requires an explanation. Kadolsky's 'belief' concerning the 'audience' is subjectivity par excellence, as are his views concerning the length of names and 'precedence as name based on a little known locality'.

5. Because of conceptual and methodological innovations, systematics has gained new respect among biologists in the scientific world today. We systematists could easily lose that respect by falling prey to prioritists' dogmatism in nomenclature.

Comment on the proposed conservation of the specific name of *Aplysia juliana* Quoy & Gaimard, 1832 (Mollusca, Gastropoda)

(Case 2949; see BZN 52: 21–23)

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I am writing to support the application to conserve the name *Aplysia juliana* Quoy & Gaimard, 1832 for the sea hare which is found worldwide in warm waters.



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