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Comments on Ph.D. thesis

Unveiling hidden species diversity in desmids (Desmidiaceae, Viridiplantae)

by Jan Šťastný (Charles University, Prague)

Overall assessment

The thesis under consideration is a sound, well-documented work. In particular the link between molecular and (traditional) morphological data makes the separate contributions in question most valuable. It has to be stressed that the significance of those contributions highly depends on a reliable identification of the algal species identified. This is often a hard job requiring a lot of experience but the candidate succeeded in a commendable way. In addition to that, the candidate made optimum use of the technical tools necessary to represent his findings in a clear and convincing way. Therefore, the thesis under discussion may be considered a model example of modern approach of microalgal taxonomy on species level. It reveals, among the desmid taxa investigated, a high degree of pseudocryptic and cryptic diversity and supports the so-called 'endemicity model' in the geographical distribution of desmid taxa.

In conclusion, I consider the above-mentioned thesis, fulfilling the criteria necessary for obtaining the Ph.D. degree, suitable for the defence.

Questions to the defendant

Paper II (*Cosmarium gauthierae* sp. nov., described from Macedonia)

I reviewed the manuscript of this paper and, on that occasion, I pointed at the high morphological similarity with a Colombian desmid, in Coesel (1992) labeled *Cosmarium dimaziforme* var. *undulatum* (compare your Fig. 7 with Fig. 17/18 in Coesel, 1992). Why was this reference not included in your final paper? I agree with you that *C. gauthierae* most likely represents a separate species but I miss suggestions concerning its closest relatives, their geographical distribution patterns and, in view of that, a possible evolutionary pathway.

Paper III (new and remarkable desmids from Europe)

Actinotaenium riethii sp. nov. is stated to represent another taxon than *Actinotaenium curtum* var. *globosum* Wille (1879). However, the only difference noticed is in their (mutually adjacent) cell dimensions, so a little reliable differentiating feature. No doubt, your *A. riethii* has nothing to do with the nominate variety of *A. curtum* as originally described by Brébisson in Ralfs (1848) but the statement (p. 296) that 'our material obviously represent a taxon different from *A. curtum* var. *globosum*' seems indefensible.

Cosmarium cinctutum Nordstedt 1875 and *Cosmarium basiornatum* (Grönblad 1926) Coesel 1998: one and the same species or two different species?

Whereas the undersigned is of opinion that we have to do with synonyms you state that two different species are at issue. Morphological differences (in your paper, p. 301, left-hand

column, l. 5 described as 'subtle') would be in the outline of the semicell, the shape of the cell sinus and the cell length to breadth ratio. However, in the original description of *C. cinctutum* by Nordstedt (1875) this species is illustrated by two figures. One of these (Plate 7: 20a) fits your characterization of *C. cinctutum*. the other one (Plate 7: 20a) your characterization of *C. basiornatum*. When claiming *C. basiornatum* to be a separate species you consider, without any substantiation, Nordstedt's Fig. 20a' a possible artefact. Please, explain!

Paper IV (polyphasic evaluation of *Xanthidium antilopaeum* and *Xanthidium cristatum*)
X. antilopaeum var. *basiornatum* is said to be principally characterized by horizontal series of supraisthmial cell wall pits (p. 406). However, neither your LM (Fig. 2E) nor your SEM picture (Fig. 4E) shows that cell wall sculpture. I can agree with your proposal, on the basis of molecular differences, to separate your strain H17 from *X. antilopaeum*, but is it justified to consider it identical to the traditional *X. antilopaeum* var. *basiornatum*?

X. antilopaeum sensu lato, strain ASW 07106, according to your Figs 3C and 5C, in my opinion morphologically agrees with *X. antilopaeum* var. *hebridarum* (see, e.g., floras West & West, Coesel & Meesters). So, why 'could not be determined' (p. 407)?

Paper V (phylogeny of baculiform desmids)

In this paper, the hypothesis by Coesel & Van Geest (2008) that all described *Triplastrum* species probably are identical is neither invalidated, nor considered unlikely. So, why not used the oldest epitheton, i.e., *T. abbreviatum* introduced by Turner (1892) instead of *T. simplex* (Allorge 1924)?

Paper VI (species delimitation within *Micrasterias fimbriata* and *M. rotata*)

You found an interesting difference in geographical distribution between the two phylogenetic lineages of *M. fimbriata* discovered. One of these seems to be confined to Atlantic regions, the other one is particularly encountered in central and eastern Europe but, surprisingly, also in Ireland. Do you have any explanation for the latter phenomenon?

Ralfs' (1848) original illustration of *M. fimbriata* corresponds to your B-lineage.

Your A-lineage morphologically does not fit any of the known varieties of *M. fimbriata*. So, in view of the convincing morphological and molecular differences detected, why not described it as a completely new taxon?

Paper VIII (evaluation of *Micrasterias crux-melitensis*/*M. radians* complex)

In your *trnG^{unc}* phylogenetic tree (p. 707, Fig. 2B) next to the strains analyzed by your own research group also a *M. crux-melitensis* strain from Genbank (FN562171) is involved originating from a Japanese culture collection (NIES 152). According to Fig. 2B, this strain is closer affiliated to the analyzed African *M. radians* var. *evoluta* strains than to your European-N. American *M. crux-melitensis* strains! Did you check the morphology of the NIES strain? If so, was it according to 'typical' *M. crux-melitensis*? If not examined, why not?

On p. 712, left-hand column, at the top it is stated that *M. crux-melitensis* comes to the fore as 'genetically homogenous'. Is this consistent with the deviating position of the above-mentioned strain NIES 152?

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Dr Peter F.M. Coesel

