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Biogeosciences Discussions

Interactive comment on "Precambrian palaeontology in the light of molecular phylogeny – an example: the radiation of the green algae" *by* B. Teyssèdre

Anonymous Referee #4

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The paper uses a comparison between molecular phylogeny and the Precambrian fossil record to examine the radiation of the green algae (Viridiplantae). The author, using time constraints from the fossil record and topology inferred from molecular phylogeny, concludes that the Chlorophyceae separated from the Ulvophyceae before 750 Ma, the Chlorophyta separated from the Streptophyta before 1200 Ma, and that the last common ancestor of the Viridiplantae and the Rhodophyta is perhaps two billion years in age.

In general, studies that combine evidence from molecular evolution with time-calibration derived from the fossil record (i.e., Peterson et al., 2004; Peterson & But-

terfield, 2005) can be very illuminating. However, the relatively sparse and sometimes contentious fossil record of early eukaryotic organisms must be handled with care, and comparisons with inferences derived from molecular data must be made cautiously.

-Page 3128, lines 12-15: The author states that "The envelope of acritarchs that withstand such a drastic treatment [i.e., HF dissolution] must possess some biopolymer like the sporopollinins or the algaenans, that are today almost exclusively typical of the Viridiplantae. So the last common ancestor of the Viriplantae and the Rhodophyta was perhaps two billion years old." It seems to me to be a bit of a stretch to conclude, simply on the basis of resistance to HF degradation, that these acritarchs are composed of a specific biopolymer diagnostic of a particular phylogenetic group. Couldn't they be composed of any number of HF-resistant organic polymeric materials (perhaps even diagenetically polymerized organic remains)? The claim that the last common ancestor of these two groups is two billion years old strikes me as being rather drastic given this piece of evidence.

-Page 3130, lines 1-18: It is quite possible that I am missing something here, but I'm not sure I understand the author's argument for the timing of the main split within the clade Pyramimonadales. The author states the following: "...this clade subdivides into two parts. The first one unites Pyramimonas with Pterosperma (probably with Pachysphaera too, but no molecular analysis of this genus is available today). The second unites Halosphaera with Cymbomonas." At issue is the split between these two lineages. The author then states that "Tasmanites is a fossil parent of the living Pachysphaera if we judge by the pores that pierce its wall and Pterospermella is an ancestor of the living Pterosperma if we judge by the membranous equatorial "wing" of its shell", and further that "...some acritarchs extracted from Thule ca. 1200 Ma were identified by Samuelsson [et.] al. (1999) as Tasmanites, some as Pterospermella, and that each of these two [taxa] may have had forerunners going back to 1350 or even 1500 Ma." Incidentally, there are two typos here (corrected in brackets), and it would be nice to have a reference for the last statement about the forerunners of Tasmanites

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and Pterospermella. Finally, the author states that "...the plentiful spheromorphs of Thule described as "Leiosphaeridia" included almost certainly some phycomas (albeit not necessarily related to Halosphaera)." The statement in parentheses is key here, because until this point the author has only provided evidence for the presence of one lineage within Pyramimonadales (in the form of Tasmanites and Pterospermella). The only evidence the author cites for the existence of the other clade is the inference that some of Thule's "Leiosphaeridia" "almost certainly" included some phycomas, and that these phycomas are related in some way to Halosphaera. However, both Pterosperma and Pachysphaera produce phycomas, according to the author, so I am left with the feeling that there is no convincing evidence for the divergence of these two lineages in the mid-Proterozoic.

-Page 3132, lines 11-17: The author states that "A phylogenetic dendrogram shows that, starting from Palaeastrum or from Proterocladus, we must cross at least twelve nodal points in order to reach the last common ancestor of the Viridiplantae. So the fact that these two [taxa] were dated ca. 750 Ma does not mean that the most ancient fossils of green algae are 750 Ma old, as Knoll stated (2003). Instead their presence shows that the radiation of the multicellular green algae started long before 750 Ma, and that the radiation of the unicellular green algae is even much older." Again, I think "taxa" would be preferable to "taxons" (corrected in brackets). I get the sense here that the author is seeking to draw a correlation between the number of bifurcations between two lineages on a phylogeny and evolutionary time. It should be acknowledged that the presence of a particular fossil provides an upper limit on its age (in other words, if Palaeastrum and/or Proterocladus were demonstrably present at 750 Ma, then it is reasonable to infer that these organisms had some period of evolution prior to this time), but I would be hesitant to draw strong inferences with respect to evolutionary timescale based on the number of nodal points between two lineages in the absence of other supporting evidence.

-Page 3134, lines 7-8: The author, referring to the Pyramimonadales found at Thule

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and the Zygnematales found at Ruyang, states that "...these fossils, albeit unicellular, are beyond any doubt genuine green algae." This may very well be true, and frankly it is beyond my ken to rigorously evaluate the validity of this claim, but it seems to me that an assertion of this magnitude deserves a reference or two or at least some kind of justification.

Interactive comment on Biogeosciences Discuss., 4, 3123, 2007.