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Biogeosciences Discussions

Interactive comment on "Lacustrine mollusc radiations in the Malawi Basin: experiments in a natural laboratory for evolution" *by* D. Van Damme and A. Gautier

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This paper promotes a very radical point of view on in-lake evolution in Lake Malawi and other East African Rift Lakes in general. This is not necessarily bad, however, this paper just promotes an opinion because virtually no data are presented by the authors. Many of the claims made by the authors vastly overshoot our current knowledge on the mollusk faunas of Lake Malawi and the other rift lakes. Almost half of the sentences in this paper could probably be listed as examples, but I will restrict myself here to some examples from the abstract. The authors suggest that 'Molluscan biostratigraphy situates this freshwater lake either in the East African wet phase between 2.7-2.4

Ma or that of 2.0-1.8 Ma'. However, no biostratigraphic framework for the mollusks of the Malawi Basin exists, and the authors do not propose such a framework in their paper nor do they provide an elaborate discussion of faunal correlations with other regions in Africa where absolute dating provides more time control. The authors claim that 'the lacustrine Chiwondo fauna went extinct at the beginning of the Pleistocene', but no conclusive evidence is provided for complete extinction of the lacustrine fauna (which moreover seems to contradict the data presented in earlier reports [Schultheiß et al. 2009]), nor for the timing of it at the beginning of the Pleistocene. The authors continue that 'The Modern Lake Malawi malacofauna is poor and descends from ubiquistic South-East African taxa and some Malawi Basin endemics that invaded the present lake after the Late Pleistocene mega-droughts', but they provide no evidence on faunal affinities of the Malawi taxa in a larger biogeographic framework (rigorous phylogenetic studies have not been undertaken for most of the genera involved, and the authors do not report new morphological or phylogenetic data on this issue here). Data that would demonstrate invasions after these Late Pleistocene calamities is also not provided; these are just-so stories. Similarly, the claim that the vast Lavigeria clade in Lake Tanganyika is 'geologically young' (which is vague; for example 30Ma can be considered 'geologically young', but drastically exceeds the average lifespan of a genus [Benton, 2009]) remains unsupported by data. More unsupported (and rampant) claims are scattered throughout this paper; one of the highlights (p.18524) is the title 'The palaeontological data reviewed: the end of Lake Malawi as an ancient lake'. First, this paper can hardly be considered a 'review' of the palaeontological data and second 'the end of Lake Malawi as an ancient lake' conveniently discards that no matter what definition of 'ancient lake' one adopts (be it based on geological age or on the extant biodiversity) the lake remains old (>100ka) and it contains hundreds of endemic species in vertebrates, invertebrates, algae and plants (e.g. Brooks, 1950; Michel, 1994; Snoeks 2000; Chafota et al. 2005)!

Moreover, the authors regularly make contradictory claims. For example, the abstract contains the following claims: 'The lacustrine Chiwondo fauna went extinct at the beginning of the Pleistocene' and 'The Modern Lake Malawi malacofauna is poor and descends from ubiquistic South-East African taxa and some Malawi Basin endemics that invaded the present lake after the Late Pleistocene mega-droughts.' However, elsewhere (p. 18529) the authors suggest: 'Taxa such as Melanoides (Thiaridae) and Gabbiella (Bithyniidae) tolerate relatively high salinity concentrations and taxa such as Lanistes (Ampullariidae) and Chambardia (Iridinidae) are able to aestivate during extended dry periods (Van Damme, 1984). Populations of these taxa may have survived the Pleistocene salinity crises and Modern representatives in the basin or in the lake such as Lanistes ellipticus, Gabbiella stanleyi and Chambardia nyassaensis may derive from basin endemics already present during Chiwondo times.'. How does this count as extinction? For example, Bellamya capillata and Lanistes ovum occur in Lake Malawi today, and the authors invoke that these taxa did so at the time of deposition of the Chiwondo Beds too (Table 1). These taxa are hence part of the lacustrine fauna that supposedly would have gone extinct, but the authors subsequently claim that faunal continuity is possible. Another example of contradiction to the abovementioned claims from the abstract comes from the conclusions on page 18533. The authors claim that: 'The available data indicate that probably already during Late Pliocene times a marked basin endemism had developed in the Malawi malacofauna…'. However, if we compare the modern fauna with that of the Chiwondo Beds in Table 1, more endemic species occur in the modern fauna (n=24) than in the mollusk fauna of the Chiwondo Beds (n=8 or 9). So how does this relate to the claim that the modern mollusk fauna of the Malawi Basin is poor? Moreover, I would be very interested to know how the authors discern basin endemics from lacustrine endemics in the geographically-restricted deposits of the Chiwondo Beds. Note that these contradictory claims are not concerned with remarks along a sideline of the paper; they relate to the main message the authors want to disperse, so what is readership supposed to make of this paper if the authors contradict themselves several times over the course of a few pages?

Additionally, the authors stack unsupported claims on top of one another to finally come to very vague, broad and almost certainly uninformative suggestions. This 'stacking'

starts with very questionable correlations related to the climate change history of the Malawi Basin. These lie at the core of this paper, for they provide the basis for the 'paleoenvironmental reconstruction' in which all assertions related to the natural history of the mollusk fauna are framed. From a massive body of literature on the timing of low and high water stands of Lake Malawi emerges the general trend that lake level oscillations from Lake Malawi are out of phase with those from African lakes farther north (see e.g. Finney et al. 1996; Gasse, 2000; Johnson et al. 2002; Filippi and Talbot, 2005). However, the authors have chosen to ignore this body of climate studies on Lake Malawi (indeed, beyond Cohen et al. [2007] not a single reference is made to this literature!). Instead the authors have chosen to adopt climate records from lakes in the northern part of the East African Rift (Trauth et al. 2005, 2007, 2010) and an unsupported correlation with lakes in the Malawi Basin from this. After all their speculation the authors come to the conclusion that (p. 18533): 'the Terminal Pliocene-Early Pleistocene aridity crises had continent-wide impact on the African malacofauna and this geological abrupt event did initiate major extinctions as well as radiations.' How many mollusk genera did go extinct on the African continent? How many families? Where do the records that would support this claim come from; do the authors for example have fossil mollusks of relevant age (and dating accuracy) from the Sahara, sub-Saharan West Africa, Southwest Africa? Virtually no information is presented in the paper. Nevertheless, the authors consider it appropriate to 'propose that the beginning of the Pleistocene is used as a reference point in calibrating molecular clocks for African freshwater mollusc phylogeny instead of estimates of the age suggested for the earliest formation of a lake in any given rift basin.'. A calibration of what exactly? As a reference point to what? The authors continue that perhaps the Gelasian Stage, certainly the Calabrian stage, late Early Pleistocene times or even Middle to Late Pleistocene dates would be consistent with molecular clock divergence time estimates. Again, divergence time estimates of what? And are the authors really promoting their proposed calibration by arguing that 'it would be consistent with currently existing molecular clock divergence time estimates'? In short they claim that the results justify the method, or more boldly stated: choose the calibration that provides you with the age you desire to obtain. Moreover, although the authors start out with the proposition to use the beginning of the Pleistocene as calibration point, their next sentence claims that pretty much the entire Pleistocene is considered 'a feasible calibration point'. None of the authors' final suggestions are in any way informative; the authors seem to consider it feasible to ignore that different organisms (that is both within mollusks and beyond) have very different evolutionary histories, biological properties and different mutation rates for the same gene. With their suggestion on how to calibrate molecular clocks they also assume that climatic changes and possible environmental calamities occur uniformly over the entire African continent and that these had a uniform effect on the mollusk fauna (which is an assumption for which no or insufficient data are provided here). Given the above and given that the authors are paleontologists, one would assume that they would appreciate the value of fossil calibration points over all their uninformative suggestions stated above.

That said, I fail to see any merits in this paper that would warrant its publication in Biogeosciences. At the bottom of this review you will find a direct appraisal using the evaluation criteria adopted by Biogeosciences. It is difficult to make suggestions as to how this paper could be fixed, for it is essentially a construct of assumptions, unsupported claims, questionable correlations, and extreme extrapolation. Several of these are even stacked on top of one another (see above), resulting into the fiction this manuscript is.

Finally, to give the manuscript a scientific appearance the authors chose to adopt an elaborate system of pseudo-referencing. For example: 'The quite impressive Holocene radiation of Lavigeria in Lake Rukwa, when this lake was joined with Lake Tanganyika (Cox, 1939; Cohen et al., 2010)...'. There is no evidence for a surface level connection uniting Rukwa and Tanganyika in one lake (see e.g. Delvaux et al., 1998). Cox did not suggest this, and although Cohen et al. (2010) is not in the reference list, I am quite sure that this report does not claim these lakes to have been united either. Fifteen

percent of the references in the reference list refer to grey or otherwise unavailable literature. For several of these, better support to the claims is provided in published reports. In several cases the claims that the present authors' make are not substantiated by the report they cite. The referencing in general is very sloppy and incomplete. Citations in the text and in the reference list regularly do not match. For example, on page 18521 a total of 16 references are cited, but only 11 of these are in the reference list.

DOES THE PAPER ADDRESS RELEVANT SCIENTIFIC QUESTIONS WITHIN THE SCOPE OF BG? THE TOPIC IS RELEVANT.

DOES THE PAPER PRESENT NOVEL CONCEPTS, IDEAS, TOOLS, OR DATA? NO, THE AUTHORS SUGGEST THEY PROVIDE A REVIEW, BUT IN FACT THIS IS AN OPINION PAPER.

ARE SUBSTANTIAL CONCLUSIONS REACHED? THERE IS NO DATA SUPPORT FOR THE CONCLUSIONS THE AUTHORS MAKE.

ARE THE SCIENTIFIC METHODS AND ASSUMPTIONS VALID AND CLEARLY OUTLINED? NO, SEE ABOVE.

ARE THE RESULTS SUFFICIENT TO SUPPORT THE INTERPRETATIONS AND CONCLUSIONS? THERE ARE NO RESULTS, THERE IS VIRTUALLY NO DATA.

IS THE DESCRIPTION OF EXPERIMENTS AND CALCULATIONS SUFFICIENTLY COMPLETE AND PRECISE TO ALLOW THEIR REPRODUCTION BY FELLOW SCIENTISTS (TRACEABILITY OF RESULTS)? THE PAPER IS BUILD ON ASSUMPTIONS, NOT ON DATA FOR WHICH REPRODUCIBILITY STUDIES COULD BE UNDERTAKEN.

DO THE AUTHORS GIVE PROPER CREDIT TO RELATED WORK AND CLEARLY INDICATE THEIR OWN NEW/ORIGINAL CONTRIBUTION? NO, THEY OFTEN CITE GREY LITERATURE WHERE OTHER, AVAILABLE LITERATURE PROVIDES BET-

TER SUPPORT. THE AUTHORS HAVE MISINTERPRETED SOME OF THE PUBLISHED LITERATURE (SEE ABOVE).

DOES THE TITLE CLEARLY REFLECT THE CONTENTS OF THE PAPER? YES.

DOES THE ABSTRACT PROVIDE A CONCISE AND COMPLETE SUMMARY? THE ABSTRACT IS TECHNICALLY SOUND, THE INFORMATION PROVIDED IS NOT.

IS THE OVERALL PRESENTATION WELL STRUCTURED AND CLEAR? THE PRESENTATION IS OK, HOWEVER THE CONTENT IS NOT CLEAR GIVEN THAT THE AUTHORS CONTRADICT THEMSELVES REGULARLY THROUGHOUT THE PAPER.

IS THE LANGUAGE FLUENT AND PRECISE? NO, THE WRITING IS TECHNICALLY OK, BUT THERE IS MISINTERPRETATION AND THE MEANING OF SENTENCES IS REGULARLY UNCLEAR (SEE ABOVE).

ARE MATHEMATICAL FORMULAE, SYMBOLS, ABBREVIATIONS, AND UNITS CORRECTLY DEFINED AND USED? THIS CRITERION DOES NOT APPLY.

SHOULD ANY PARTS OF THE PAPER (TEXT, FORMULAE, FIGURES, TABLES) BE CLARIFIED, REDUCED, COMBINED, OR ELIMINATED? SEE ABOVE.

ARE THE NUMBER AND QUALITY OF REFERENCES APPROPRIATE? NO, THE REFERENCING IS SLOPPY (SEE ABOVE).

IS THE AMOUNT AND QUALITY OF SUPPLEMENTARY MATERIAL APPROPRIATE? NO SUPPLEMENTARY MATERIAL IS PROVIDED, THIS CRITERION DOES NOT APPLY.

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