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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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Response to the comments made by Bert Van Boxclaer

1. *Concerning the (supposed) absence of a biostratigraphic framework.*
R: a solid framework using Corbicula and Pseudobovaria is presented. The Asian Corbicula has been used as guide fossil since the 1980'ths, included by Van Boxclaer.

2. *Concerning our (untrue) claims that the Modern Malawi fauna is poor and derived from Pleistocene invaders.*
R. The low species richness of the Modern L. Malawi malacofauna is a solid fact. With a total of 35-39 species (uncertain number of Melanoides species) the species

richness of this enormous lake greatly differs from the only bona-fide ancient lake, Lake Tanganyika (> 100 species, according to the latest estimates) and it is even lower than the very young Lake Victoria (45 sp). It actually has about the same species richness of the equally young L. Chad (35 sp), which dried completely out during the last glacial. As to the biogeographic origin of Pleistocene invaders in Lake Malawi, most of the Modern Malawi non-endemics species do have a wider distribution in southern Africa, some, considered formerly as endemic, also extending north in the Tanganyika basin (see our Table 1). That also is an undeniable fact. Invasions hence may have come from the north as well as from the west (related species in the Mweru-Bangweulu region) as from the south (Lower Zambesi region). The molecular evidence also suggests that the three endemic clades are relatively young. The geological evidence indicates extreme aridity from the onset of the Pleistocene hence a discontinuity of lacustrine freshwater ecosystems and therefore also inevitably leads to an invasion scenario.

3. *Concerning our (supposedly unsupported) claim that the vast Lavigeria clade in Lake Tanganyika is 'geologically young'*
R. We refer e.g. to Wilson et al. (2004). In their figure of 'Neighbor-joining distance tree for 495bp of 16S alignment of Tanganyikan gastropod dataset' the branch length distance among Lavigeria species is less than among Melanoides tuberculata populations. Other references and additional arguments are also cited in support in our article.

4. *No matter what definition of 'ancient lake' one adopts . . .Lake Malawi remains old (>100ka) (in contrast to what we state).*
R: This is a purely semantic argument in which the accent is shifted from the intended concept 'freshwater lake and its malacofauna' to 'lake sensu lato' (lifeless or close to it, e.g. Modern Lake Turkana). Extreme low level stands in Lake Malawi are generally accepted to have occurred during LGM-times, hence about 25.000 years ago. If a water body did exist during that time in the Malawi basin it must have been restricted to the deep northern basin and that much smaller lake must have been alkaline/saline

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(ion concentrations during such lower stands can be extrapolated based on data of recent ion-concentrations, the (considerable) residence time, temperature and evapo-transpiration rates, see e.g. Branchu et al., 2010).

5. *Concerning our supposedly contradictory statements about faunal extinctions and persistence in the Lake Malawi Basin'.*
R. There is no contradiction. In our opinion is it a case of (possible) exceptions proving the rule. We cited these possible exceptions in order to present a varied and hence more complete picture instead of trying to make our scenario of extinction as solid as possible.

6. *'More endemics are present in the Modern Lake Malawi fauna than in the Chiwondo fauna'*
R. Van Boxclaer possesses sufficient first-hand information to know perfectly well that the fossil assemblages from Chiwondo only represent a fraction of the whole Early Pleistocene malacofauna. We clearly state that this is the case, citing the causes. We do possess a very incomplete picture of the past, probably of the most common larger species only.

7. *'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. . .. . . the authors have chosen to ignore this body of climate studies'*
R. We indeed do not discuss these articles (as we did not discuss most other post-glacial climatic reconstructions) because they are not relevant on the time scale used in our article. The difference in phasing cited in the articles mentioned is a well-known phenomenon that is valid on a time scale of hundreds of years, to become much weaker on a time scale of millennia of years and that is of no significance over periods of ten thousand or hundred thousand of years. Van Boxclaer is well aware of that.

8. *'Finally, to give the manuscript a scientific appearance the authors chose to adopt an*

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elaborate system of pseudo-referencing. For example: '. . .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.

R. Cox (1939), having identified Rukwa fossils as belonging to Tanganyikan thalassoid genera, notes: 'These determinations undoubtedly support the view that direct connections existed between the two lakes'.

Cohen et al., (2010) write: 'Here we report on fossil evidence from the Lake Rukwa Basin, which indicates a former hydrologic connection with downstream Lake Tanganyika'. During the Early Holocene L. Rukwa overflowed in Lake Tanganyika via the Karema Gap as can be observed via satellite imaginary. Recent geological evidence of this well-known connection (see Van Damme, 1984) is presented in: Fernandez-Alonso, M., Delvaux, D., Klerkx, J. and Theunissen, K.: Structural link between Tanganyika- and Rukwa-Rift Basins at Karema-Nkamba (Tanzania): basement structural control and recent evolution. Roy. Mus. Cent. Africa, Tervuren (Belgium), Geological Department, Ann. Rep. 1999-2000, 91-100, 2001.

When Van Boxclaer and I went to the British Museum several years ago in order to discuss possible joint projects with Jonathan Todd and Ellinor Michel, this subject was extensively broached because I really had wanted to establish a joint investigation. Van Boxclaer's present absolute rejection of this Rukwa-Tanganyika connection, without even consulting the literature, is remarkable.

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