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Discussions

# *Interactive comment on* "Constant diversification rates of endemic gastropods in ancient Lake Ohrid: ecosystem resilience likely buffers environmental fluctuations" *by* K. Föller et al.

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This paper makes for an highly interesting contribution to the long-standing discussion on diversification in ancient lakes, particularly given the new insights from scientific drilling campaigns that have raised the awareness of the severeness of fluctuations in crucial environmental parameters during the last (few) million years, e.g. in Lake Malawi. Focussing on the most diverse animal clade in Lake Ohrid, Europe's only ancient lake, this MS provides important biological baseline data for interpreting the impact of environmental fluctuations as inferred from a (rather recent) excellent coring record that is still being analyzed. A few issues should still be addressed, though, in



order to further improve the MS. As the first reviewer, A. Weigand, has already pointed out, it would be interesting to see whether the apparent support for the monophyly of the non-pyrgulinid Hydrobiidae in Lake Ohrid by a BPP of 0.87 is also found when using a ML analysis, or, as I would also suggest, a BI analysis without constraints. In contrast to the authors' statement that they ran (initially) unconstrained analyses, I would suggest that any molecular clock analysis, whether relative or calibrated, is constrained in a way by the requirement of ultrametry. Given that BEAST enforces bifurcations, a, say, MrBayes analysis would show whether the topology and support for it are stable. Based on the present tree, I cannot quite agree with the first referee on his second point, as collapsing basal (unsupported) splits would not contradict the onset of diversification in non-pyrgulinid hydrobiids before deep-water conditions set in at Lake Ohrid. Regarding the inference of rate homogeneity and LTT plots (also discussed by A. Weigand), a brief perusal of Ricklefs 2007 suggests to me that this point certainly warrants some more discussion by the authors, acknowledging potential limitations and alternative hypotheses. I also concur with the other points raised by the first referee. Anyhow, I am convinced that the concerns raised above can be dealt with either by providing evidence for them being unfounded and/or by providing a more balanced discussion - either way, the data are worthy of publication in this journal.

Interactive comment on Biogeosciences Discuss., 12, 14271, 2015.