**Biogeosciences
Discussions**

Interactive comment on "Spatially explicit analyses of gastropod biodiversity in ancient Lake Ohrid" *by* T. Hauffe et al.

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Main comments

The ms describes gastropod biodiversity patterns in the ancient lake Ohrid. Biodiversity was measured as species richness and species composition, describing the fundamental aspects of biodiversity within and among sampling locations. The authors applied state of the art multivariate techniques, and spatially explicit methods.

Exploring the relative significance of different mechanisms that generate diversity patterns in a well defined natural entity is an outstanding opportunity, although totally observational. Nevertheless, replication within the lake system enabled the authors to obtain good quality biotic data and to perform a complicated correlational analysis.

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The most important results are that (i) homogeneity of species composition varies with depth, deeper layer faunas being more homogeneous due to less fluctuating environmental conditions, while surface layer faunas bear the imprint of a more fluctuating and spatially heterogeneous environment. (ii) Conclusions regarding the early signs of biotic homogenization (regionally common species spreading from around developed areas, and lower levels of endemisms than previously found) together with the observation that newly invading species cannot colonize deeper water areas in contrast with endemics with wide depth tolerances is a reason for some relief that invasion does not threaten the endemic fauna directly. But it is known that invasive molluscs often are altering biogeochemical systems through their massive abundances (Potamopyrgus, Dreissena), consequently might indirectly threaten other species. So the false illusion of "endemics are fine" as a conclusion should be avoided by augmenting the argument.

I feel that the manuscript is overly technical relative to its important and interesting topic. A whole arsenal of techniques is used, but the real story is somewhat hidden behind technical jargon. This might subtract readers from fully comprehending the message. A suggest to the Editor that a minor revision of the manuscript should address some issues with respect to specific technical details of the analysis, and the text should be revised to make the topic more accessible to readers not familiar with spatial methods. Most technical details (what function was used in which R package) can be placed in an Appendix. [footnote: it is rather exceptional to give full credit to all R packages used.]

Specific comments

The word 'analyses' in title should be 'analysis'

It is not clear if relative abundances were used for calculating Bray-Curtis dissimilarities. It is important to give some hints about the relative magnitude of abundances in each rank categories, because it does matter if ranking was applied (1) to reflect differences in magnitudes (1, 10, 100 inds.) or (2) because of inaccuracy of the abundance

measure. This should be clarified. Strictly speaking, ordered ordinal scaled variables are not really suitable for calculating Bray-Curtis dissimilarities unless an underlying ratio scale measurement exists.

I might have missed it, but I can't see area corrected endemic richness appearing in the results.

I can't always see the clear distinction between elements of the paper dealing with species composition and species richness. From a methodological point of view, these approaches require different methodologies. While using NMDS along with Bray-Curtis dissimilarities and PCNM is totally legitimate for multivariate data sets, i.e. for multiple response variables, these are not optimal choices for univariate modeling situations.

The choice of permutational multivariate ANOVA for the univariate analysis of species richness is opaque. Similarly, the use of multivariate spatial filtering technique for univariate non-independence case is unjustified. Spatial autocorrelation of the univariate residuals after regressing for environmental covariates can tell if autocorrelation is still significant. If it is the case, some autoregressive model (SAR, CAR) could be employed. If I misunderstood the text, it needs clarification.

The use of the null modeling approach to validate the NMDS stress value is not indicative to the goodness of mapping of the original dissimilarities onto the 2-3 dimensional space. This piece can be dropped from the manuscript, because irrelevant. A null distribution of stress values tells nothing about the goodness of the observed statistic, because the observed statistic is a final result of an iterative procedure minimizing an objective function. Thus there is no need for other justification. Parameter free methods do not require parameters.

Spatial autocorrelation in the spatial distribution of species richness is not an indication of biotic interactions. To study biotic interactions, the proper modeling of species identities required as opposed to lumping them together in a richness measure. (Inhibition and mutual stimulation might refer something different than competition and facilitation,

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but if this is true, the authors must define what do they mean by these terms).

The rue potential of the data lies in differentiating between environmental (depth layers, horizontal habitat classes) and purely spatial drivers of biodiversity (variation partitioning of partial Mantel tests could have been used to measure the relative significance of these factors). This should be reflected more intensively in the text.

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