

Interactive
Comment

***Interactive comment on “ Eutrophication and
warming effects on long-term variation of
zooplankton in Lake Biwa” by C. H. Hsieh et al.***

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Reviewer 2 agrees very much with our analyses and results. The comments are editorial. We have made all those editorial corrections and improvements.

The comments from the reviewer are copied below in bold, followed by our replies and changes in the manuscript.

The paper presents an interesting analysis of a long-term database of zooplankton and environmental data (including phytoplankton) obtained in the largest lake in Japan: Lake Biwa. The paper is globally well written and the analyses

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and methods developed are appropriate. However the relatively high number of methods used by the authors and the different initial matrices (obtained after the aggregation of original data), I suggest that the authors make a general schematic representation of their methodology (i.e., a global diagram showing all steps of the analyses and the connections between them). When the methodology will be clear and the options used at each step justified (as mentioned in MM section) this will help the reader to capture the key messages from the methodological aspects.

A schematic illustrating analytical procedures is added as Figure 4 of the revised version. It is attached in the reply. Justification had been included in MM section as the reviewer noted.

The results obtained by the RDA analyses are very interesting. They show the importance of the aggregation of either phytoplankton or zooplankton data. But it is surprising that environmental factors explained the highest percentage of variance of zooplankton matrices. This result could be due to the construction of the different matrices in this study. I do not think that using only phytoplankton biomass (even if taxonomic groups were separated) is the best descriptor for zooplankton dynamics. It is better to involve additional factors (cell size, presence or not of colonies, etc.) for trophic interactions. Moreover the authors confirmed a change of the trophic status of the lake during eutrophication and re-oligotrophication processes. Maybe the use of the complete time series is not appropriate. In addition the phytoplankton time series is short (1978-2003) compared to zooplankton time series (1962-2005).

We do not fully understand this comment. Indeed, cell size and colony or not (morphology) of phytoplankton had been considered in our original analyses. Results are shown in Tables 4 to 6 and supplementary information B. We add on page 19, line 12-14: “However, one should note that these variance partition analyses were done based

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on data only from 1978 to 2003; this limited data series may hamper our resolution of analyses.”

It is clear that some changes on species composition and/or their phenology occurred during this period. Even if the paper focused on the inter-annual variability it would be nice to know (at least in the discussion) if some phenological changes and/or species composition occurred during the study period. We need such additional ecological information to try to better explain the observed pattern.

We agree that considering phenology will be beneficial. However, we have only quarterly zooplankton data, which makes phenological analyses not easy, although we are still doing the analyses. Our current manuscript is already complicated in analyses. We rather prefer concentrating the analysis on phenology in the next manuscript. We add on page 16, line 16-20: “Furthermore, we noted several zooplankton and phytoplankton taxa in Lake Biwa have exhibited phenological shifts during the past several decades. These shifts may cause trophic mismatch and result in community changes (Anneville et al., 2002b; Anneville et al., 2002a). Detailed analyses on phenology of zooplankton is beyond the scope of this study but should be a next critical step.”

The ratios of zooplanktonic groups used here are good indicators of environmental variability. What are the consequences on carbon fluxes? Because lakes can be considered as simplified oceanic systems (a very simplistic representation) what can we learn from this analysis to improve (or criticise) existing biogeochemical models (i.e. NPZ family models)? If the authors include such aspect in the discussion the paper will be more widely interesting for oceanographers and limnologists.

Thanks for the interesting suggestion. We add on page 18, line 15-23: “Zooplankton are reasonable bio-indicators to environmental changes. Combining these data with environmental and phytoplankton data allows one to investigate the energy fluxes of

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pelagic ecosystem of Lake Biwa in the context of a changing climate. For example, a steady-state box model including bacteria, phytoplankton, zooplankton, and detritus has been developed for the pelagic ecosystem of Lake Biwa using inverse methods based on limited data collected from snapshots in summer as well as some parameters from the literature (Niquil et al., 2006). Our rich data with high temporal resolution and coverage may potentially be used to investigate how energy flows change with time, and in particular, with environmental variation.”

Minor comments Figure 3 to 5 could be combined in a single or two figures. Particularly figures 4 and 5 should make a single composite figure. Page 2, line 17: ‘affect’ instead of ‘affects’ Page 5, line 20: ‘in response to’, to is missing here

We combined figure 4 and 5. The typos are fixed in the revised version.

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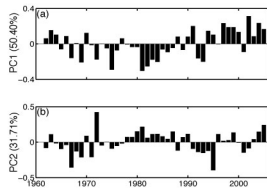
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