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## ***Interactive comment on “The last glacial-interglacial cycle in Lake Ohrid (Macedonia/Albania): testing diatom response to climate” by J. M. Reed et al.***

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Received and published: 1 August 2010

### General comments

This is an interesting, timely and well executed study. It draws together for the first time diatom evidence for environmental change in Lake Ohrid from c. 130 ka BP to present. It also attempts to incorporate into the interpretation aspects of diatom taphonomy (dissolution) and morphology (e.g. size classes) with respect to changing climate over broad timescales. More detail could be given on proposed mechanisms between climate change and diatom assemblage composition, e.g. through shorter/longer duration of ice cover, summer stratification, nutrient availability etc. Overall however, this

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study is an important benchmark for future higher resolution studies.

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Specific comments

Abstract:

informative and succinct

Introduction

P4691, c. line 11: pollen is also consistently preserved in LB sediments, but have been much less extensively studied

P4692: in the examples being given here, might be worth referring also to Rioual et al. QSR (2007) and their detailed diatom work for the period 130 – 105 ka BP and the Last Interglacial in particular at Ribain's Lake, France.

Otherwise interesting introduction which contextualises nicely the importance of the current study

Methods

I think that it would be useful to include the age model figure from Vogel et al. 2010a (JQS)

State how many samples were actually analysed for diatoms

What was the rationale between two different laboratories counting diatoms on the same samples?

A figure showing main morphotypes for each *Cyclotella* species would be a useful addition here

Why was CONISS used as a zonation technique and not arguably better techniques such as optimal partitioning (Birks & Gordon 1985)?

With regard to the ordination techniques, a little more detail/justification could be given

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for different options chosen. For example, given such a low gradient (<2.5 SD units), PCA might be a more appropriate technique, given 'arch' problems encountered using CA.

Also, why were rare species down-weighted?

### Results

P4697, line 8: replace 'CA results...' with 'Diatom results...'

How do the different datasets compare if the Macedonian categories are combined. For example, if the 8 size classes for *C. fotti* are combined to >20 and < 20 um (similar to the UK dataset?)

If the *Aulacoseira* sp. is unidentified, are the authors certain it is endemic? What other information do we have about this taxon?

P4697, lines 19+: photographs here would be useful to confirm statements linked to morphological variability

According to the CONISS output on Fig 3, the most important boundary between zones is between COD2b and COD3

The results section is a mixture of purely results, but also some interpretation as well – perhaps leave all interpretation to the discussion to avoid potential repetition? (e.g. p4698, lines 1-2)

In fig 3, given that the chronology is well established, it would be useful to either plot alongside the depths, approximate ages, or at least give ages alongside the zone boundaries

### Discussion

In fig 4, I would add in total *C. fotti* species too, as this taxon has clearly been indicated to represent cooler prevailing climate, in contrast to *C. ocellata* – or is this taxon already

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included in FP+benthic? More clarity here needed

Relating tephra layers (in cm) to age-scaled Fig 4 is not particularly useful. These observations should come in the results section

P4700, line 25+: which aspect of the geochemical record suggests a temp minimum at 108.4 ka? It is not apparent on fig 4

The corresponding 'peak' in *C. ocellata* looks to be part of a fluctuating curve, and given the resolution of the diatom analysis and uncertainty in the dating, I'm not convinced that the statement "...suggesting correlation with the North Atlantic C24 event...is unreliable." is warranted.

P4701, lines 3-7: this section seems a little confused. With regard to the Baikal comparison, diatom evidence for cooling starts at c. 119 ka BP, and minimum productivity is reached by c. 117 ka BP (Rioual & Mackay 2005; Mackay 2007). Biogenic silica evidence suggests that minimum productivity is reached by c. 115 ka BP (Prokopenko et al. 2006) with the start of MIS5d and full glacial conditions. Finally, the reference Mackay (2007) is misplaced, as it suggests that that study was related to Ohrid.

The diatom data for MIS 3 are indeed very interesting. Clearly, the zonation marked between COD3 and COD4 highlights the potential correspondence between diatoms and the start of MIS3.

Throughout the whole of the MIS3 (i.e. zone COD4), the most characteristic feature is the presence of *S. pinnata* – could the authors expand more on its significance during this period? for example, could it be that ice cover duration declined enough to allow a littoral community to establish in the lake?

Finally, given that the authors have compared their study to Baikal for MIS5e/5d, they could also compare their data for MIS 3 in Baikal (Swann et al. 2005). For example, Swann et al. also demonstrate diatom evidence for warm conditions between c. 39-35 ka BP. However in Baikal peak warming was probably during the start of MIS 3 between

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c. 54-51 ka BP, so here the Ohrid and Baikal records appear to differ substantially.

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P4702, lines 16-17: the authors could be more specific for dates for the start of late glacial warming (c. 14.7 ka BP) and the start of the Holocene (c. 11.7 ka BP), given the resolution they ascribed to earlier transitions during e.g. MIS 5e and MIS 3.

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P4702, lines 26-27: does the sediment record show evidence for IRD during the early Holocene as well?

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P4702, lines 27+: could it be that the diatoms are not as sensitive to changes in climate as the authors suggest, rather than the other way round? The resolution of the profile overall is rather low, and does not easily allow transition periods or abrupt changes to be interpreted in general (which the authors themselves acknowledge on page 4703).

P4704, lines 10-12: ought it be implicit that size classes are a feature of evolutionary pressures?

Conclusions are appropriate

References used in the review but not in manuscript.

Birks, H.J.B. & Gordon, A.D. 1985. Numerical methods in Quaternary pollen analysis: London, Academic Press.

Prokopenko, A.A., Hinnnov, L.A., Williams, D.F. & Kuzmin, M.I., 2006. Orbital forcing of continental climate during the Pleistocene: a complete astronomically tuned climatic record from Lake Baikal, SE Siberia. *Quaternary Science Reviews* 25, 3431–3457.

Rioual, P., Andrieu-Ponel, V., de Beaulieu, J.L., Reille, M., Svobodova, H. & Battarbee, R.W. 2007. Diatom responses to limnological and climatic changes at Ribains Maar (French Massif Central) during the Eemian and Early Würm. *Quaternary Science Reviews* 26, 1557-1609.

Rioual, P. & Mackay, A.W. 2005. A diatom record of centennial resolution for the Kazantsevo Interglacial stage in Lake Baikal (Siberia). *Global and Planetary Change*

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46, 199-219.

Swann, G.E.A., Mackay, A.W., Leng, M.J. & Demory, F. 2005. Climatic change in Central Asia during MIS 3/2: a case study using biological responses from Lake Baikal. *Global and Planetary Change* 46, 235-253.

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Interactive comment on *Biogeosciences Discuss.*, 7, 4689, 2010.

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