

## ***Interactive comment on “The environmental and evolutionary history of Lake Ohrid (FYROM/Albania): Interim results from the SCOPSCO deep drilling project” by Bernd Wagner et al.***

**P.C. Tzedakis (Referee)**

p.c.tzedakis@ucl.ac.uk

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The MS presents a synthesis of initial results of the SCOPSCO deep drilling of Lake Ohrid project, previously published in a series of papers in Biogeosciences. It brings together information from the four main aims of the project (age and origin of lake; seismotectonic history; volcanic activity and climate change; biodiversity and endemism) and compares results from different types evidence and approaches. As such, the whole is greater than the sum of its parts and the study is of great value to the scientific community. The text is well-written and organized and the figures of excellent quality. I

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have one substantive comment and one minor quibble.

1. A potentially important conclusion emerging from several strands of evidence is a long-term trend from cooler and wetter to drier and warmer glacials and interglacials, starting at ~300 ka. However, closer examination reveals that the trends between different types of evidence are not always congruous.

Water depths estimated from seismic data suggest a decrease in lake levels from 300 ka, but the trend is reversed from MIS 4 to today, with water depths increasing. The authors suggest (p. 17, l. 22) that this is in broad agreement with regional vegetation trends inferred from pollen analysis, but this not entirely accurate: the pollen data show that the two driest periods were the penultimate (MIS 6) and last glacial (MIS 4-2). This is mainly based on the large expansions of *Artemisia* during these intervals, vis-à-vis very low values in earlier glacials (incidentally, a feature that has not been observed in other long pollen sequences).

The claim that pollen data and inferred water depths show parallel trends is repeated on p. 20, but, again, if water depths increase from MIS 4, then there is divergence between the two over this interval. The pollen data suggest that in addition to glacials, a drying trend is also observed in interglacials. This is mainly based on the reduction of montane tree values in MIS 5 and MIS 1 (especially the almost complete disappearance of *Picea*; though *Fagus* increases somewhat from MIS 5c onwards) (Sadori et al., 2016). On the other hand, Mediterranean taxa percentages don't show any trend apart from a brief maximum at the MIS 4/3 boundary (which is unexpected), so it might be more useful to show the montane taxa in Fig. 4.

The drying trend theme is picked up again on p. 21, with the oxygen isotopic evidence. More specifically, a trend towards higher interglacial  $d_{18}O$  in endogenic calcite after 300 ka is invoked, but close inspection shows that it is only MIS 5 that shows that; Holocene values are not that different from earlier interglacials. Interestingly, the  $d_{18}O$  record from siderite shows lowest values in the penultimate and last glacials. This is

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interpreted (p. 21, 30-31) as evidence for lower evaporation during glacials, which is reasonable. However, it is also interpreted as a “higher influence of winter precipitation (increased seasonality), which supports the interpretation of the palynological record”. This, in fact, appears at odds with the high Artemisia expansions.

In conclusion, while the inference of a drying trend is potentially a very interesting and exciting observation, I would suggest that a more nuanced interpretation is needed, as close inspection reveals a more complicated picture amongst the different lines of evidence.

2. Referencing appears somewhat idiosyncratic at times, with an overall tendency to cite recent works. Thus, on p. 23 the attribution for the work on D/O and Heinrich events of the last glacial should include the original papers by Bond et al. (1992, 1993, *Nature*) and Dansgaard et al. (1993 *Nature*), while for older glacials McManus et al. (1999, *Science*), Raymo et al. (1998, *Nature*) and Barker et al. (2011, *Science*), probably deserve a mention. On the same page, (l. 12-13), important papers on the impact of HE and D/O events include Shackleton et al. (2000 *Paleoceanography*), Roucoux et al. (2001, *QR*), Margari et al. (2010 *Nature Geoscience*).

While the need to limit the overall number of references in a work of this wide scope is understandable, the paucity of references on the body of work on the environmental impacts of North Atlantic millennial-scale variability in the Balkans seems to be an oversight (e.g. Tzedakis et al., 2002, *Science*, 2004, *Geology*; Margari et al., 2009 *QSR*; Müller et al., 2011 *QSR*; Roucoux et al., 2011 *JQS*; Fletcher et al., 2013 *QSR*).

Finally, on the fascinating topic of the reservoir vs cradle function of Lake Ohrid, it might be worth recalling that that local buffering from extreme environmental effects in refugial areas may have not only led to reduced extinction rates, but also allowed lineage divergence to proceed, and thus refugia may have acted both as ‘museums’ for the conservation of diversity and as ‘cradles’ for the production of new diversity (Tzedakis et al., 2002 *Science*; Tzedakis, 2011 *J. of Biogeogr.*).

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p. 14, l. 5 References for Lake Ioannina?

MPT: At several places mention is made of the ‘end of the MPT’. Could this be more specific?

In sum, this is an extremely useful work and I am happy to recommend publication, subject to minor revision, which is needed to address the issues raised above.

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