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Interactive comment on "Mediterranean climate since the Middle Pleistocene: a 640 ka stable isotope record from Lake Ohrid (Albania/Macedonia)" *by* J. H. Lacey et al.

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General comments

Lacey et al. present the results of extensive stable carbon and oxygen isotope analyses of a long sediment sequence, spanning multiple glacial-interglacial cycles (up to MIS 16) from Lake Ohrid (Albania/Macedonia) as part of the Scientific Collaboration on Past Speciation Conditions in Lake Ohrid (SCOPSCO) project. This is the most extensive and highly resolved lacustrine isotope record from this region, and, to my knowledge, one of only several of its kind globally. This research is of international importance, on account of (i) the relative rarity of long terrestrial palaeoclimate records from this region

that encompass multiple glacial cycles; (ii) the important location of this site, at the interface of temperate and arid climate systems; (iii) the provision of a climate context in which to evaluate speciation in an ancient lake. The manuscript is very well written and the structure adopted is effective, given the amount of grown to cover. The detailed and thoughtful account (section 6.1 and 6.4) of isotope dynamics in and around the Ohrid basin was particularly useful and demonstrates a rigorous approach to the research by the team. There are difficulties at this site in securing isotope data from most of the glacial sediment sections of comparable resolution to the interglacial sections. Nevertheless, this detailed record makes an important contribution towards understanding hydroclimate variability over multiple glacial cycles and will facilitate future efforts to investigate regional phase relationships under different boundary conditions.

Specific comments on the manuscript are provided below. A fresh pair of eyes will almost inevitably spot the odd typo / points for clarification and these are detailed under the 'technical corrections' section of the review.

Specific comments

1. It appears that the ms exclusively interprets the highly-resolved and extensive δ18O record. Although equally highly resolved and extensive δ13C data have also been collected and presented alongside the δ18O record, and the various complex controls on δ13C detailed at length, there is no interpretation of the δ13C sequence (or description of the data in the results); the palaeoclimate interpretation (detailed in section 6.4) is based exclusively on the δ18Olw record. As the δ13C data does not contribute to the story, I'm not sure why it is included. Perhaps either remove the sections on δ13C and present these results in detail in a separate paper, or at least utilise these data in support of the palaeoclimate interpretations.

2. The interpretation of the δ18Olw record and the relationship between the Ohrid isotope record and other regional climate records would benefit from further explanation. For example, there are instances where high AP frequencies at Tenaghi Philip-

pon coincide with both high δ18Olw values (e.g. MIS 7e, 7c, 7a; linked to lower P/E driven by increased evaporation) and low δ18Olw values (e.g. MIS 5e, 5c, 5a; linked to higher P/E driven by enhance precipitation). Therefore, the relationship between AP frequency and δ18Olw is complex, despite both proxies being driven by temperature and moisture. There is scope to provide more explanation to account for the δ18Olw variability and to reconcile the δ18Olw and AP / SST records. The authors touch on the role of enhanced seasonality during MIS 5, with increased winter precipitation accounting for the inferred recurrence of low δ18Olw coinciding with MIS 5e, 5c and 5a. The authors may wish to further consider the potential role of seasonality under the different boundary conditions captured in their sequence (e.g. Kutzbach et al., 2014, Climate Dynamics 42, 1079-1095) and whether this is apparent in the δ18Olw record (i.e. the relative influence of increased winter precipitation combined with enhanced summer aridity, vs. drier winters and milder summers on P/E values, as recorded in 'summer' calcite). To this end it may be worth showing summer and winter insolation curves alongside Figure 3 or Figure 8.

3. The abstract details the causes for lower δ18Olw during glacials, rather than the causes for higher δ18Olw during interglacials (which, measured by data volume, comprises most of the data of the ms). As it stands, the reader has to assume that the causes for higher interglacial δ18Olw were the opposite of the causes mentioned for the low glacial δ18Olw (i.e. warmer summer temperatures, lower proportion of winter precipitation falling as snow, and an increase inflow from Prespa). If this is the case, then there are instances, outlined above, where explanations centre on rainfall amount as being particularly important. The causes for higher δ18Olw during interglacials therefore should be detailed in the abstract.

4. In the introduction, the context of the main justification of the research (p.13430, Line 22-26) is rather brief. I feel there is scope to expand this section, e.g. perhaps by identifying links with other SCOPSCO projects and the importance of achieving a palaeoclimate context / framework to investigate the evolution of taxa in Lake Ohrid.

5. The chronology section may be better placed directly after the core recovery section. In explaining the chronology of the sequence, the relationships and assumptions involved in tuning TOC to insolation should be detailed. Furthermore, it should be clarified whether the 1k error is applicable to both the tuning approaches and the tephrostratigraphical approach.

'Technical corrections' / suggestions

Title: suggest consider Northern Mediterranean climate since the Middle Pleistocene. . ...(on p. 13429 (Line 25), you specify northern Mediterranean region here, hence the suggestion to modify the title of the ms).

Abstract

p. 13429, Line 5: suggest use the term 'composite core'

p. 13429, Line 17 & 24: suggest use δ18Olw 'values'

p.13429, Line 21: please clarify the meaning of 'isotopically freshest'.

Introduction

p.13430, Line 14: Use of the word 'confined'. Suggest re-word.

p.13430, Line 16 & p.13457, Line 24: Frogley et al., 1999 (also update ref list)

p.13430, Line 28: Please specify in what way the lake has been shown to be sensitive to millennial-scale climate variability.

p.13431, Line 6: A brief recap of the primary aims of the SCOPSCO project would be helpful here (or outlined earlier as suggested in comments above).

General setting:

p. 13431, Line 16: m.a.s.l. Please write in full on first use. (Similarly all other abbreviations should be given in full on first use, e.g. ICDP (p.13432, Line 25), DOSECC (p. 13433, Line 6), TOC (p.13435, Line 10).

p. 13431, Line 27: Water outputs are quantified, but not inflow. Do you have these data to include here?

p.13432: Suggest replace Tzedakis et al. 2009a citation here with something more appropriate (e.g. Harding, A., Palutikof, J., Holt, T., 2009. The climate system. In: Woodward, J. (Ed.), The Physical Geography of the Mediterranean. Oxford University Press, Oxford, pp. 69–88).

p.13432, Line 22: 'winds trace the Ohrid valley'. The meaning is a little unclear; suggest clarification.

Material and Methods:

p.13432, Line 25: Typo 'different 4 sites'

p. 13433, Line 7: Explain what is meant by 'complete composite', e.g. how many core locations contributed to the composite core?

p. 13433, Line 8: core 'material'

p. 13434, Line 26: ground 'to a fine powder'.

Chronology

p.13435, Line 10: Please specify the 'TOC related proxies'.

p.13435, Line 26: For clarity / accuracy, suggest reword 'covers' to 'broadly corresponding to'

p.13436, Line 1: Following on from above, for clarity / accuracy suggest a caveat is included to highlight that terrestrial and marine chronostratigraphies are independent.

Results

p.13436, Line 4: Details of this core should be provided in the materials and methods section (see comments above).

p.13436, Line 8: I appreciate for the sake of brevity that MIS numbers are used throughout. However, for clarity I would suggest some additional wording, e.g. 'The sediments corresponding to MIS 15 and 13. . ..', at least on first use of the MIS terminology.

Structure: suggest detail TIC results first (as this is related to MIS). In this context, a brief explanation of calcite / siderite formation would be helpful here.

p.13436, Line 18: Could be more precise here; calcite is present in MIS 14 and 16.

p.13436, Line 25: More description of isotope variability between glacial stages in required here (e.g. similarities / differences), or if the record is of insufficient resolution for this, then this should be stated here.

Discussion

p.13437, Line 8: specify which datasets are being referred to.

p.13437, Line 16: suggest quantify Ohrid and Prespa average isotope compositions for comparison.

p.13437, Line 19: δ18O precipitation (δ18Op), i.e. give in full on first use.

p.13437, Line 25-28: suggest re-word, the meaning a little unclear.

p.13437, Line26: use of word 'only' when in fact it is the majority.

p.13438, Line 4: do you mean a uniform composition in δ18O?

p.13440, Line 15: Please provide more details (e.g. frequency / core location) of the SEM investigations used to infer the morphological characteristics of the core material.

p.13441, Line 1-4: Suggest re-word for clarity, e.g. '..would require early Holocene lake water temperatures > 5oC cooler than present'.

p.13441, Line 10: suggest re-word to 'largely restricted' (i.e. to account for the presence of δ18O calcite data from MIS 13-16).

p.13441, Line 18: suggest reword ‘anti-correlate’ – do mean inversely correlated?

p.13444, Line 4: please explain here why the Zhang et al. (2001) solution, as opposed to Carothers et al. (1988), is more appropriate for defining equilibrium precipitation at lower temperatures.

p.13444, Line 19: please qualify the use of the term ‘fresher’. The suggestion here (and a few lines later) is that Ohrid is behaving as a closed-system, with ‘fresher’ (higher P/E) conditions during glacials and more saline (lower P/E) during inter-glacials. Is there corroborating evidence that this is the case (e.g. biological proxies?). Perhaps more appropriate to talk in terms of a semi-closed system during inter-glacials, and more open during glacials?

p.13447, Line 6: ‘inflow δ13C’, suggest reword for clarity, e.g. inflow of δ13CTDIC from springs etc.

p.13447, Line 22 and elsewhere: for clarity, please refer consistently to ‘high’ and ‘low’ δ, rather than ‘light’ or ‘heavy’ values, or positive / negative excursions etc.

p.13447, Line 29: for clarity suggest change to. . . Lake Ohrid δ13CTDIC

p.13448, Line 1: Perhaps be a little more cautious here. Yes I agree you would expect poor soil development and open landscapes during glacials, but the pollen evidence cited in support only extends back to 92ka. Similarly, the assertion that deciduous trees would have dominated during warmer intervals is presented without empirical evidence from this site. Therefore, reference should be made to the nearby Tenaghi Philippon AP record of Tzedakis et al. (2006) here.

p.13448, Line 12: ‘. . .enough time is available’. Could you be more precise in defining how long?

p.13448, Liner 17:. . .may also reflect. . .

p.13449, Line 6: Typo - on rather than of

p.13450, Line 13 and elsewhere: suggest use GHG ‘concentrations’ rather than ‘content’

p.13450, Line 18: suggest re-word from ‘extended’ to ‘multiple glacial / inter-glacial’

p.13451, Line 10: Do these excursions in Ohrid δ18Olw correspond to MIS sub-stages?

p.13451, Line 18: It would be useful to refer to Figure 7 here.

p.13451, Line 24: suggest qualify / re-word the statement ‘full interglacial conditions’ when used in the context of MIS 14.

p.13452, Line 5: It would be useful to refer to Figure 7 here.

p.13452, Line 11: Typo – LR04 that. . .

p.13453, Line 15-20: This may benefit from discussion in the context of core recovery / integrity at this interval.

p.13453, Line 25: δ18Olw minimum reached earlier at around c.380-375 ka

p.13453, Line 27: in comparison to the first half of MIS 11.

p. 13454, Line 10-15: The description and explanation of the δ18Olw record I found a little difficult to follow. Perhaps this is an artefact of the highly-resolved record, but I could only see one major excursion to low δ18Olw values at c.318 ka (rather than at c. 324 ka which are higher) and so it seems that the warmest and most evaporative conditions occur at c. 318 ka. This appears to coincide with MIS 9d and a drop in AP at Tenaghi Philippon. If this is the case, then why would low AP values coincide with high δ18Olw at Ohrid?

p.13454, Line 12: Query whether you mean lower δ18Olw at c.318 ka.

p.13454, Line 21: For clarification suggest re-word. . .The δ18Olw data between 291 and 281 ka. . ..

p.13454, Line 24: Suggest re-word to ‘relatively low’ to better reflect the rather inter-

mediate values represented at the boundary in comparison to the rest of the core.

p.13454-p.13455, Line 27-2: Requires rewording as the meaning is unclear.

p.13455, Line 19: suggest change to 'previous interstadial substage'

p.13456, Line 14: suggest re-word 'short-lived' and provide the approximate duration of this sub-stage.

p.13456, Line 15-16: suggest reword as unclear.

p.13457, Line 2: typo – as rather than a

p.13457, Line 8-18: There are new and detailed regional palaeorecords from MIS 5 that you may want to consider (e.g. Martrat et al., 2014, Quaternary Science Reviews 99, 122-134; Grant et al., 2012, Nature 491, 744-747; Marino et al., 2015, Nature 522, 197-201).

p.13457, Line 24: If comparing to Ioannina, please see the latest paper on the MIS 6/5e transition, with its revised chronology (Wilson et al., 2015, Geology 43, 818-822). See Martrat et al., 2014 (Quaternary Science Reviews 99, 122-134) for a more detailed account of SST variability during this interval.

p.13458, Line 17: query whether you mean 5d rather than 5b.

p.13458, Line 18: query whether you mean 5b rather than 5d.

p.13458, Line 19: suggest quantify length of sub-stage rather than using the term 'short-lived'

Figures

Figure 3: The axis scaling adopted is unclear and makes it difficult to read off the values. Specify it is calcite isotope data.

Figure 4: Please write in full before using abbreviations.

Figure 8: Need to make it clear that LR04 is plotted on an inverted axis and clarify whether it is the δ18Olw running mean that is plotted.

Interactive comment on Biogeosciences Discuss., 12, 13427, 2015.