

The authors would like to thank Dr. Graham Wilson and an anonymous reviewer for their positive comments and constructive suggestions on our manuscript, “Mediterranean climate since the Middle Pleistocene: a 640 ka stable isotope record from Lake Ohrid (Albania/Macedonia)”. Below we provide a point-by-point reply to each comment.

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## G. Wilson (Referee #1)

### Specific comments

1. It appears that the ms exclusively interprets the highly-resolved and extensive  $\delta^{18}\text{O}$  record. Although equally highly resolved and extensive  $\delta^{13}\text{C}$  data have also been collected and presented alongside the  $\delta^{18}\text{O}$  record, and the various complex controls on  $\delta^{13}\text{C}$  detailed at length, there is no interpretation of the  $\delta^{13}\text{C}$  sequence (or description of the data in the results); the palaeoclimate interpretation (detailed in section 6.4) is based exclusively on the  $\delta^{18}\text{O}_{\text{lw}}$  record. As the  $\delta^{13}\text{C}$  data does not contribute to the story, I’m not sure why it is included. Perhaps either remove the sections on  $\delta^{13}\text{C}$  and present these results in detail in a separate paper, or at least utilise these data in support of the palaeoclimate interpretations.

- The  $\delta^{13}\text{C}$  record is extensive and highly-resolved, complementing  $\delta^{18}\text{O}$ , and is important to the interdisciplinary work of the SCOPSCO project. The manuscript has been revised accordingly and we now present and discuss both  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$

2. The interpretation of the  $\delta^{18}\text{O}_{\text{lw}}$  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 Philippon coincide with both high  $\delta^{18}\text{O}_{\text{lw}}$  values (e.g. MIS 7e, 7c, 7a; linked to lower P/E driven by increased evaporation) and low  $\delta^{18}\text{O}_{\text{lw}}$  values (e.g. MIS 5e, 5c, 5a; linked to higher P/E driven by enhanced precipitation). Therefore, the relationship between AP frequency and  $\delta^{18}\text{O}_{\text{lw}}$  is complex, despite both proxies being driven by temperature and moisture. There is scope to provide more explanation to account for the  $\delta^{18}\text{O}_{\text{lw}}$  variability and to reconcile the  $\delta^{18}\text{O}_{\text{lw}}$  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  $\delta^{18}\text{O}_{\text{lw}}$  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  $\delta^{18}\text{O}_{\text{lw}}$  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.

- There is an interesting link between  $\delta^{18}\text{O}$  and AP. Lower  $\delta^{18}\text{O}$  values correspond to periods of higher AP, which includes both MIS 5 and MIS 7. The timing of excursions will be better illustrated with the addition of  $\delta^{13}\text{C}$  data, which has a more direct relationship with AP variability (by comparison with AP from Sadori et al. 2015). We agree it would be worthwhile expanding the discussion as suggested, however given the concerns regarding the current length of the manuscript, such a detailed comparison between Ohrid and regional records may be better placed in a future manuscript specifically concerning interglacial structure and diversity. The role of seasonality under different boundary conditions is an interesting question, however with the sample material utilised here (bulk calcite, sampled at c. 500-yr resolution and with a 70-yr lake-water residence time) it would be difficult to unravel changes in seasonality. Even with increased winter precipitation, summer hydroclimate is considered (timing of calcite precipitation) and with greater winter precipitation (few per mil difference) we would expect a similar  $\delta^{18}\text{O}_{\text{lw}}$  end point (within error) due to differential evaporation rates between lower and higher  $\delta^{18}\text{O}$ . We agree it would be worthwhile showing insolation and now include this in Figure 8.

3. The abstract details the causes for lower  $\delta^{18}\text{O}_{\text{lw}}$  during glacials, rather than the causes for higher  $\delta^{18}\text{O}_{\text{lw}}$  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  $\delta^{18}\text{O}_{\text{lw}}$  were the opposite of the causes mentioned for the low glacial  $\delta^{18}\text{O}_{\text{lw}}$  (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  $\delta^{18}\text{O}_{\text{lw}}$  during interglacials therefore should be detailed in the abstract.

- We have adapted the abstract to provide an overview of the suggested controls on isotope variation between interglacial and glacial periods

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

- We have expanded the scope of the introduction and the relevance of this study to the SCOPSCO project and palaeoclimate research

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 teprostratigraphical approach.

- We agree the overview of the composite profile chronology would be better placed after the 'core recovery' section and have amended the manuscript accordingly. We now include information on the relationship between TOC, insolation and winter season length. An error of 2k is now applied to the TOC tuning points, which will be clearly stated in the manuscript. The ages and related errors of the teprostratigraphic tie points are discussed in detail by Leicher et al. (2015)

#### **'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).

- As suggested, the manuscript title has been amended

#### *Abstract*

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

- 'composite core profile' is now used in place of 'sediment cores'

p. 13429, Line 17 & 24: suggest use  $\delta^{18}\text{O}_{\text{lw}}$  'values'

- 'values' is now used on line 17 (not applicable to line 24 due to amendments from above comment)

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

- We have amended the sentence to describe a change to 'wetter' conditions during MIS 11-9 (rather than 'isotopically freshest', which was used to describe the shift to lower  $\delta^{18}\text{O}$ )

#### *Introduction*

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

- 'Confined' has been reworded

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

- Citation has been updated throughout the manuscript and in the reference list

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

- We now include examples of proxy datasets and clarify the lake is sensitive to both long- and short-term climate variations

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).

- The introduction has been amended to include the primary SCOPSCO aims

#### *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).

- Abbreviations are now written in full on first use

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

- Water inflow is now quantified

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).

- Citation has been replaced as suggested

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

- This has now been clarified in the text

#### *Material and Methods*

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

- Typo has been corrected

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

- Text has been amended to explain the composite profile and further differentiate it from the drilling results

p. 13433, Line 8: core 'material'

- Text added

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

- Text added

#### *Chronology*

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

- Text has been added to specify the 'TOC related proxies' (i.e. TOC/TN)

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

- Text replaced as suggested

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.

- The added text (comment above) highlights that only a broad correlation exists, and does not imply a direct association between marine and terrestrial chronostratigraphies

#### *Results*

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

- The use of Lini Co1262 is now detailed in the materials and methods section

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.

- Text added

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.

- Text has been added to summarise calcite/siderite occurrence, with reference to Francke et al. (2015)

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

- This paragraph has been amended (based also on comments from Reviewer 2) to remove the suggestion all glacial/stadial periods are characterised by siderite

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

- Sentence added to highlight variability between glacial stages, and the resolution of the siderite record has been addressed

### *Discussion*

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

- The citation now directly refers to the modern water dataset

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

- Values for Ohrid and Prespa are now provided

p.13437, Line 19:  $\delta_{18}O$  precipitation ( $\delta_{18}Op$ ), i.e. give in full on first use.

- Text added

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

- Text relating to spring water input has been modified

p.13437, Line 26: use of word 'only' when in fact it is the majority.

- Amended as part of corrections based on the comment above

p.13438, Line 4: do you mean a uniform composition in  $\delta_{18}O$ ?

- Yes, ' $\delta_{18}O_w$ ' added

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.

- Text modified

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

- Text modified as suggested

p.13441, Line 10: suggest re-word to 'largely restricted' (i.e. to account for the presence of  $\delta_{18}O$  calcite data from MIS 13-16).

- Text modified as suggested

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

- Yes, text has been amended

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.

- This paragraph has been modified, also taking into account comments from Reviewer 2

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?

- The text now clarifies the calcite-siderite comparison. The modern water balance (detailed under 'General setting') shows the lake is hydrologically open today, and water input was likely reduced during glacial periods. It is therefore perhaps not appropriate to use the terms 'open' and 'closed' as higher P/E during glacials Lake Ohrid may not necessarily be due to a change in hydrological closure status

p.13447, Line 6: 'inflow  $\delta^{13}\text{C}$ ', suggest reword for clarity, e.g. inflow of  $\delta^{13}\text{C}$ TDIC from springs etc.

- Text modified

p.13447, Line 22 and elsewhere: for clarity, please refer consistently to 'high' and 'low'  $\delta^{13}\text{C}$ , rather than 'light' or 'heavy' values, or positive / negative excursions etc.

- Text has been adapted throughout the manuscript

p.13447, Line 29: for clarity suggest change to: : : Lake Ohrid  $\delta^{13}\text{C}$ TDIC

- Text modified

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.

- We now cite pollen evidence from the composite profile (Lake Ohrid core 5045-1; Sadori et al., 2015)

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

- The section on  $\delta^{13}\text{C}$  has been updated based on Reviewer 2 comments

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

- Text modified

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

- Text modified

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

- Text modified throughout the manuscript

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

- Text modified as suggested

p.13451, Line 10: Do these excursions in Ohrid  $\delta^{18}\text{O}$ lw correspond to MIS sub-stages?

- The discussion has been modified,  $\delta^{13}\text{C}_c$  also shows a clear pattern of variability consistent with MIS substages

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

- Text now refers to Figure 7

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

- Text has been modified

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

- Text now refers to Figure 7

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

- Text modified

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

- Manuscript modified, also in line with comments from Reviewer 2, and no longer applicable

p.13453, Line 25:  $\delta^{18}O_{lw}$  minimum reached earlier at around c.380-375 ka

- The discussion has been modified, incorporating the updated age model of Francke et al. (2015)

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

- Text modified

p. 13454, Line 10-15: The description and explanation of the  $\delta^{18}O_{lw}$  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  $\delta^{18}O_{lw}$  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  $\delta^{18}O_{lw}$  at Ohrid?

- The discussion has been revised, where the inclusion of  $\delta^{13}C_c$  highlights the MIS substages ( $\delta^{18}O_c$  and  $\delta^{13}C_c$  are also closely coupled through much of MIS 9). The driest conditions occur at ca. 319 ka, however this is interpreted as the stadial phase 9d (supported by low TIC, bSi, AP and higher K). P/E will be influenced by water throughput, as well as temperature/evaporation effects

p.13454, Line 12: Query whether you mean lower  $\delta^{18}O_{lw}$  at c.318 ka.

- This has been revised based on the above comment

p.13454, Line 21: For clarification suggest re-word: : :The  $\delta^{18}O_{lw}$  data between 291 and 281 ka : : ..

- Text modified

p.13454, Line 24: Suggest re-word to 'relatively low' to better reflect the rather intermediate values presented at the boundary in comparison to the rest of the core.

- Text modified

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

- Text reworded

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

- Text modified as suggested

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

- Text modified

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

- Text modified

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

- Section reworded, not applicable

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).

- We have considered the suggested records, however a detailed study of MIS 5 is for future consideration (with a higher sampling resolution)

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.

- As suggested we now refer in this section to the latest paper with the revised chronology

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

- 5d, text modified

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

- 5b, text modified

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

- We have updated this section

### 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 3 has been replotted, in line with comments from both reviewers to improve clarity

Figure 4: Please write in full before using abbreviations.

- We explain abbreviations in the figure caption

Figure 8: Need to make it clear that LR04 is plotted on an inverted axis and clarify whether it is the  $\delta^{18}\text{O}_{\text{lw}}$  running mean that is plotted.

- We include  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  calcite data (showing both raw and smoothed data) and note that LR04 is plotted on an inverted axis in the figure caption

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### Anonymous (Referee #2)

#### Specific Comments

I find it somewhat circular to compare the results of Ohrid to LR04 when much of the record from Ohrid has been tuned to LR04. While I don't consider this a fatal flaw, some acknowledgement to this effect would be beneficial in the discussion.

- The LR04 3<sup>rd</sup> order tuning points have been removed from the age model, based on reviewer comments to Francke et al. (2015). The age model is now based on tephrochronology and tuning TOC (and TOC/TN) to orbital parameters

I also think that using a range of temperatures and thus a range of calculated (not modelled)  $\delta^{18}\text{O}_{\text{lw}}$  would be a more appropriate way to interpret the data. Although using modern temperatures is fine as a first approximation, taking the average value of 18°C may not be appropriate for later time intervals. Thus the interpretation that evaporation increases in later interglacials, such as MIs 7, 5, may be erroneous and the result is from temperature change during peak calcite formation.

- We agree that using a single temperature estimate may not be appropriate for calculating  $\delta^{18}\text{O}_{\text{lw}}$  in previous warm stages (bottom water temperature used for siderite will be less variable). We now use a range of temperatures to better illustrate the sensitivity of our calculations to variations in temperature and incorporate this into a revised Figure 3. For calcite data we use +18°C and give range between 15°C and 21°C (to account for a proportion of variability between interglacial/interstadials). For siderite we use 5.8°C and give a range between 4°C and 7°C (assumes a consistently deep lake). Importantly the relationship between interglacial and glacial  $\delta^{18}\text{O}_{\text{lw}}$  remains unchanged. It is also important to note that temperature changes are unlikely to be a primary driver of  $\delta^{18}\text{O}_{\text{c}}$ . The equilibrium fractionation between calcite and water has a gradient of approximately  $-0.24\text{‰}/\text{°C}$ , which is directly opposed by the change in  $\delta^{18}\text{O}_{\text{precipitation}}$ . Although there are no monitoring stations in FYRO Macedonia, data from regional GNIP stations (e.g. Thessaloniki and Patras, Greece) suggest a gradient between approximately +0.2 and +0.3‰/°C. Therefore any overall temperature effect will be small (assuming these relationships have not changed significantly between warm stages). This is an

interesting comment and highlights the importance of obtaining an independent proxy for temperature change at Lake Ohrid

Pg. 13436 Ln 22 and Pg. 13442 Ln 11-13: Methods do not need to be reiterated. You have already stated that the siderite was confirmed by multiple methods (XRD, FTIR, etc).

- We agree and have removed later references to the methodology for identifying siderite

Section 6.3.2. This section should be substantially reduced, particularly if most of this is published elsewhere. It adds to the length of an overly long paper. The important part is that the siderite is authigenic (early diagenetic). The geochemical constraints needed to create siderite do not need to be described in such detail. The importance of the siderite is not widely discussed later on nor is it a key component of interpretations, save to suggest that it forms during glacial stages. Anything to reduce paper length is beneficial.

- As suggested we have reduced this section and focus on the early-diagenetic nature of the siderite

Section 6.3.3 This section could also be reduced. Simply say that at low temperatures, the equation of Zhang et al. 2001 is considered the most robust (Ludvigson et al., 2013). Leave out the entire 1st paragraph.

- We agree with the suggestion and have reduced this section

Section 6.3.4. The section on the  $\delta^{13}\text{C}$  values is needed for completeness but does not need to be so great. The data are never discussed in detail after this. Yes the carbon isotopes can track sources of carbon but if those sources are not integral to the conclusions then they need not be included to this detail. In fact, this section seems to largely be a literature review of other studies. It does not have much bearing on the later climate interpretation. I think it can be streamlined considerably.

- We now incorporate  $\delta^{13}\text{C}$  into our discussion

Section 6.4 should be the main focus of the paper. However, it was difficult to follow b/c the figures that supported this discussion were too small and not properly labelled with sub-stages. Increases/decreases in  $\delta^{18}\text{O}$  values were hard to see given the figure compression. I would strongly recommend altering the figures in some way.

- We have updated both figures to make the information clearer, specifically Figure 8 has been updated to utilise  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  calcite data and now does not include glacial stages (thereby expanding the plots, improving the clarity of individual substages)

Pg. 13452 Ln. 12-14: The statement “ $\delta^{18}\text{O}$  are slightly elevated above those of MIS 13c, which suggests the latter may have had marginally higher P/E due to cooler conditions or higher annual precipitation” seems contradictory to later interpretations. If higher evaporation is responsible for increased  $\delta^{18}\text{O}$  values then why in MIS 13c is higher precipitation responsible for elevated  $\delta^{18}\text{O}$  values. I may be misreading this but if so, others will as well. It simply does not make sense to me.

- Lower P/E corresponds to higher  $\delta^{18}\text{O}$ . We have updated the text to provide better clarity (the inclusion of  $\delta^{13}\text{C}$  following reviewer comments assists with this section)

Pg. 13452 Ln 21-22: I do not understand what is meant by “artificially enhanced”.

- Wording now amended

Pg. 13459 Ln 6-9. This is not a conclusion; it is an analysis that you did. I think it is easier for the reader to remember the important parts of the paper if you simply reiterate the main points without specifically reiterating what you did.

- We agree and have amended the conclusion in line with this comment

#### *Technical Comments*

Pg. 13434, Ln. 29: comma after the word “sample”.

- Text amended



Pg. 13456 Grammatically, this sentence does not make sense to me. "MIS 7a in Lake Ohrid is short-lived and characterised by a shift to lower  $\delta^{18}O_{lw}$ , in comparison to MIS 7c following the stadial phase, that are (??) highly variable but overall increase until TIC production ceases at around ca. 200 ka.

- Text has been amended

Pg. 13457 Ln 5. I believe the Figure citation should be Fig. 3 or 8 (not 7).

- This was the correct citation; the sentence first referred to average values (Fig. 7), however we agree it would be clearer to refer to reader to Figure 8, text has been modified accordingly

### *Figures*

Fig. 1: The colours on the "bathymetric map" mean something relative to depth. This might be useful for the reader.

- We refer to 'lake-floor morphology' as described in the original reference (which does not provide a legend for the figure, however the colour spectrum applied depicts lake bathymetry in an interpretable format)

Fig. 3. Caption last sentence. Change to "calcite data are given". Also possibly break into two sections. Details are hard to see. It would also be helpful to label MIS a, b, c, d, e. You indicate that lettered sub-stages are after Rainsback, and they are discussed in text but the reader is left to determine what on the figure goes to what. Since the labels are so small it is difficult.

- Caption has been changed. We have updated the figure to include labelled substages and improved overall clarity. All figures are provided as vector artwork in PDF format to aid closer inspection

Fig. 7. Labels and dots too small.

- Size has now been increased

Fig. 8. See comments about figure 3.

- Figure 8 has been updated in line with comments for Figure 3