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.

G. Wilson (Referee #1)

Specific comments

1. It appears that the ms exclusively interprets the highly-resolved and extensive _18O record. Although equally highly resolved and extensive _13C data have also been collected and presented alongside the _18O record, and the various complex controls on _13C detailed at length, there is no interpretation of the _13C sequence (or description of the data in the results); the palaeoclimate interpretation (detailed in section 6.4) is based exclusively on the _18Olw record. As the _13C data does not contribute to the story, I'm not sure why it is included. Perhaps either remove the sections on _13C and present these results in detail in a separate paper, or at least utilise these data in support of the palaeoclimate interpretations.

- The $\delta^{13}C$ record is extensive and highly-resolved, complementing $\delta^{18}O$, and is important to the interdisciplinary work of the SCOPSCO project. We will present $\delta^{13}C$ results and revise the palaeoclimate overview to discuss both $\delta^{18}O$ and $\delta^{13}C$

2. The interpretation of the _18Olw 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 _18Olw values (e.g. MIS 7e, 7c, 7a; linked to lower P/E driven by increased evaporation) and low _18Olw values (e.g. MIS 5e, 5c, 5a; linked to higher P/E driven by enhance precipitation). Therefore, the relationship between AP frequency and _18Olw is complex, despite both proxies being driven by temperature and moisture. There is scope to provide more explanation to account for the _18Olw variability and to reconcile the _18Olw 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 _18Olw 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 _18Olw 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 $\delta^{18}O$ and AP. Lower $\delta^{18}O$ 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 $\delta^{13}C$ 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 $\delta^{18}O_{lw}$ end point (within error) due to differential evaporation rates between lower and higher $\delta^{18}O$. We agree it would be worthwhile showing insolation curves and will add to Figure 8.

3. The abstract details the causes for lower _18Olw during glacials, rather than the causes for higher _18Olw 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 _18Olw were the opposite of the causes mentioned for the low glacial _18Olw (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 _18Olw during interglacials therefore should be detailed in the abstract.

- We will adapt the abstract to provide an overview of the suggested controls on the 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 will expand 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 tephrostratigraphical approach.

- We agree the overview of the composite profile chronology would be better placed after the 'core recovery' section. We will 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 tephrostratigraphic 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 will be amended

Abstract

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

- 'composite core profile' will be used in place of 'sediment cores'

p. 13429, Line 17 & 24: suggest use _18Olw 'values'

- 'values' will be used in both cases

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

- We will amend 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 $\delta^{18}O$)

Introduction

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

- Will be reworded

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

- Citation will be 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 will include examples of proxy datasets and clarify the lake records 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 will be 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).

- Will be 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?

- All water inflow will be 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 will be replaced

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

- We will clarify the text

Material and Methods

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

- Typo will be corrected

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

- Text will be amended to explain the composite profile and further differentiate it from the drilling results

p. 13433, Line 8: core 'material'

- Text will be added

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

- Text will be added

Chronology

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

- Text will be added to specify 'TOC related proxies' (i.e. TOC/N)

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

- Text will be 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.

- Text will be added to highlight the independence of terrestrial and marine 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 will be 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 will be 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 will be 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 will be amended (based also on comments from Reviewer 2)

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.

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

Discussion

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

- The citation will directly refer 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 will be provided

p.13437, Line 19: _18O precipitation (_18Op), i.e. give in full on first use.

- Text will be added

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

- Text relating to spring water input will be modified

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

- This will be amended as part of corrections based on the comment above

p.13438, Line 4: do you mean a uniform composition in _18O?

- Yes, will add '$\delta^{18}O_{lw}$'

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 will be modified

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

- Text will be modified

p.13441, Line 10: suggest re-word to 'largely restricted' (i.e. to account for the presence of _18O calcite data from MIS 13-16).

- Text will be modified

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

- Yes, text will be modified

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 will be 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?

- We will modify the text to clarify 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 was not due to a change in hydrological status

p.13447, Line 6: 'inflow _13C', suggest reword for clarity, e.g. inflow of _13CTDIC from springs etc.

- Text will be modified

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

- Text will be modified throughout the manuscript

p.13447, Line 29: for clarity suggest change to: : : Lake Ohrid _13CTDIC

- Text will be 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 will cite pollen evidence from 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 $\delta^{13}C$ will be updated

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

- Text will be modified

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

- Text will be modified

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

- Text will be modified throughout the manuscript

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

- Text will be modified

p.13451, Line 10: Do these excursions in Ohrid _18Olw correspond to MIS sub-stages?

- We will modify the discussion, and the inclusion of $\delta^{13}C_c$ show also shows a pattern of variability consistent with that of MIS substages

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

- Text will be modified

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

- Text will be modified

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

- Text will be modified

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

- Text will be modified

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

p.13453, Line 25: _18Olw minimum reached earlier at around c.380-375 ka
- We will modify the discussion, also based on an updated age model

p.13453, Line 27: in comparison to the first half of MIS 11.
- Text will be modified

p. 13454, Line 10-15: The description and explanation of the _18Olw 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 18Olw 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 _18Olw at Ohrid?
- We agree that this is not clear and the text will be revised

p.13454, Line 12: Query whether you mean lower _18Olw at c.318 ka.
- This will be included in the revisions from the comment above

p.13454, Line 21: For clarification suggest re-word: : :The _18Olw data between 291 and 281 ka: : :..
- Text will be 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 will be modified

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

p.13455, Line 19: suggest change to 'previous interstadial substage'
- Text will be modified

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

p.13456, Line 15-16: suggest reword as unclear.
- Text will be modified

p.13457, Line 2: typo – as rather than a
- Text will be modified

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).
- The suggested records will be considered

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.
- The suggested records will be considered

p.13458, Line 17: query whether you mean 5d rather than 5b.
- Text will be modified

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

- Text will be modified

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

- Text will be modified

Figures

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

- We will replot figure, in line with comments from both reviewers

Figure 4: Please write in full before using abbreviations.

- We will provide abbreviations written in full within the figure caption

Figure 8: Need to make it clear that LR04 is plotted on an inverted axis and clarify whether it is the _18Olw running mean that is plotted.

- We will include $\delta^{18}O$ and $\delta^{13}C$ calcite data (showing both raw and smoothed data) and will 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 3rd 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/N) to orbital parameters

I also think that using a range of temperatures and thus a range of calculated (not modelled) _18Olw 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 18oC 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 $\delta^{18}O_{lw}$ in previous warm stages (bottom water temperature used for siderite will be less variable). Using a range of temperatures will better illustrate the sensitivity of the calculation to variations in temperature and we will incorporate this into a revised Figure 3. Using a range of ±3°C for calcite data and ±2°C for siderite (very much an upper estimate) the relationship between interglacial and glacial $\delta^{18}O_{lw}$ remains unchanged. It is important to note that temperature changes are unlikely to be a prominent driver of $\delta^{18}O_c$. The equilibrium fractionation between calcite and water has a gradient of approximately –0.24‰/°C, which is directly opposed by the change in $\delta^{18}O_{precipitation}$. 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 will remove 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 will reduce 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 reviewers suggestion and will condense this section

Section 6.3.4. The section on the _13C 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 carbo 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 will now incorporate $\delta^{13}C$ 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 _18O values were hard to see given the figure compression. I would strongly recommend altering the figures in some way.

- We will edit both figures to make the information clearer, specifically Figure 8 will be updated to use $\delta^{18}O$ and $\delta^{13}C$ calcite data and will not include glacial stages (thereby expanding the plots, making substages easier to see)

Pg. 13452 Ln. 12-14: The statement "_18Olw 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 _18O values then why in MIS 13c is higher precipitation responsible for elevated _18O 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 $\delta^{18}O$. We will update the text.

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

- We will update the wording here for clarity

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 will amend the conclusions in line with this comment

Technical Comments

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

- Text will be 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 _18Olw, 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 will be amended

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

- This is the correct citation; the sentence first refers to average values (Fig. 7), and then specific excursions (Fig. 8)

Figures

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

- We will 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 Railsback, 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 will be changed. We will update the figure to include labelled substages and improve overall clarity. All figures will be provided as vector artwork in PDF format for ease of closer inspection.

Fig. 7. Labels and dots too small.

- Labels sizes will be increased

Fig. 8. See comments about figure 3.

- Figure 8 will be updated in line with comments for Figure 3