

# ***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.***

## **Anonymous Referee #2**

Received and published: 20 October 2015

### General Comments:

A wealth of new data are presented in this paper. The record is long and there is a good deal of information to be gleaned here. The task to sort through these data is impressive in its scope and the authors have done a solid job of integrating their data with previous studies for the northeastern Mediterranean sector. However, I struggled to keep my attention and focus. My single biggest concern is that this paper is excessively long –although it is in an acceptable format for the journal. However, one must read 23 pages before they actually get to the climate discussion promised in the introduction. The additional information is interesting, but not always necessary and can detract from the reader’s ability to focus on the major points. In general, I felt as if I

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was reading a PhD dissertation. The methods seemed overly detailed, as did some of the background information. (Please see specific comments on streamlining the text). Many ideas were discussed that seemed to derive from other papers but did not contribute to the climate interpretation. I also found the figures to be difficult to read. The labels on the figures were very small—at least in the version I had. I read it as a pdf. The isotope profiles were so long and compressed that the variations discussed in the text were difficult to see. I would prefer that the both Fig. 3 and 8 be split so that the y-axis could be stretched out more and the labels could be bigger. This is an excellent record so it is to the authors' advantage to maximize the readers ability to study the data.

With respect to the specific questions posed to reviewers:

1. The authors posed no specific question(s) in the paper. This is simply a description of results found and interpretation of those results. However, if they presented specific questions, the paper might be easier to follow and the information transferred to the reader may be more memorable. For example: a) MIS 11 is considered to be the most similar to the current interglacial in several respects. Does this similarity hold true at Lake Ohrid? b) Stage 5e, 5c, and 5a are clearly visible in vegetation records in Greece and the surrounding area. Are these expressed in the water of Lake Ohrid and do the isotopes mirror the variations in P/E suggested by the vegetation? (These are just off-the-cuff questions. Better ones could surely be formulated by the authors).
2. The paper presents a wealth of new data extending the knowledge of climate variability during the latter part of the Pleistocene. These data are valuable and will contribute significantly to our understanding of long term climate variability of the region.
3. Yes—to an extent.
4. Scientific methods are clearly outlined and justified. Perhaps these can be trimmed up a bit. (see specific comments)
5. Discussion had apparent contradictions—perhaps it was my misread. (See specific comments.)
6. Results are traceable.
7. Yes, although I would point out that there are many basic references about isotopic systems that are older and just as good. The authors tend to

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self-cite. The paleolimnology community has been discussing isotopes in lake systems since the 1970s (e.g. Stuiver, 1970). Landmark papers (e.g. the review by Talbot 1990) were produced in the 1980s, 1990s. As cited, it would seem that we have only known about isotopes and lacustrine carbonates since the 2010s. 8. Yes 9. OK 10. I found the paper difficult to follow. This was less a function of structure and more a function of length and redundancy. 11. Yes 12. Yes 13. Yes, reduction is needed. See specific comments. 14. Yes

#### 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.

I also think that using a range of temperatures and thus a range of calculated (not modeled)  $\delta^{18}\text{O}_{\text{lw}}$  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.

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).

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.

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.

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.

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 labeled 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.

Pg. 13452 Ln. 12-14: The statement “ $\delta^{18}\text{O}$ lw 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.

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

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.

Technical Comments:

Technical comments are few. The paper is well written.

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

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.

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

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

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.

Fig. 7. Labels and dots too small.

Fig. 8. See comments about figure 3.

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

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