

MS No.: bg-2015-303 MS Type: Research Article Special Issue: Integrated perspectives on biological and geological dynamics in ancient Lake Ohrid.

Title: Improved end-member characterization of modern organic matter pools in the Ohrid Basin (Albania, Macedonia) and evaluation of new palaeoenvironmental proxies.

Author(s): J. Holtvoeth, D. Rushworth, A. Imeri, M. Cara, H. Vogel, T. Wagner, and G.A. Wolff

### **General comments:**

The manuscript entitled “Improved end-member characterization of modern organic matter pools in the Ohrid Basin (Albania, Macedonia) and evaluation of new palaeoenvironmental proxies” represents a very detailed, molecular-level characterization of organic matter from within the lake system and its watershed. The work is primarily based on biomarker analysis and includes the application/development of novel molecular proxies which have ample potential to be applied under different scenarios. The authors are clearly highly experienced in this field and have ample knowledge and prior research expertise working on paleo-records of Lake Ohrid. As such, they are very familiar with the pertinent literature, which is properly represented in the manuscript. The manuscript is well written (a bit long), the data seem very solid, and the interpretations proper within the limitations indicated by the authors. The ‘improved end-member characterization’ leading to the ‘evaluation of new paleoenvironmental proxies’ is not only ideally suited for the special issue on ‘Integrated perspectives on biological and geological dynamics in ancient Lake Ohrid’, but also provides perspectives applicable on a much wider basis for the paleoenvironmental community. As such I rate this contribution highly and recommend its publication in Biogeosciences. Below, I list some minor-to-moderate comments which might help improve the manuscript.

- 1) Line 13, page 8: “Free and bound acids”? How were these obtained?
- 2) Line 9, page 15: The authors suggest that Taraxerol is ‘quickly degraded’ based on their data. This is very surprising since this compound has been widely applied as a marker for mangrove detritus, and has been shown to be quite stable in the environment. The authors should revise this comment as it is misleading.
- 3) Page 19, lines 3-14: I suggest introducing the concept of Paq here. In fact, I feel that the Paq data should be shown in a Table or graph. The fact that the Paq values in the sediments are so low is a clear indication of a very limited contribution of macrophytes and floating/submerged plant materials to the sediments. This is particularly important during time periods when the lake water level decreased, which may suggest a vegetation shift along the lake edges to include larger macrophyte contributions. However, this doesn’t seem to be the case. So, may be making more intensive use of the Paq information may be merited.
- 4) I am surprised how much effort went towards stable C and H isotope analyses, and how little was gained from this effort. I would imagine that even if there was a range of values in the end-members, that a significant climate shift would be reflected in a change in primary productivity (i.e.  $\delta^{13}\text{C}$  shift) and a change in hydrology or water stress (i.e. shift in  $\delta\text{D}$ ). While I found the discussion on the stable isotope data a bit confusing, I realize that it would take significant additional experimental efforts to constrain the findings and place them into a paleo-environmental context. However, I wonder if it would be worth shortening this section considering that the paper is already quite extensive? Also, with this section in mind – what about effects of primary productivity changes to explain the data discussed in lines 15-20 on page 33? Or, what were the n-alkane specific  $\delta\text{D}$  values during ‘warmer/humid’ vs. ‘cooler/drier’ climates (Page 35, lines 25-28)? Do they agree with your statements?

5) Line 14, page 32: remove 'always' – is confusing.

6) Line 26, page 42: The authors suggest that adding bulk carbon and hydrogen stable isotope data to lipid biomarker data might help resolve issue regarding terrestrial vs. aquatic OM pools. I tend to agree with that, but in their case the stable isotope data did not seem to make a difference. As such it seems a bit awkward to state this here.

**Recommendation:** Publish with minor-to-moderate changes.