Biogeosciences Discuss., 12, C7982–C7988, 2015
www.biogeosciences-discuss.net/12/C7982/2015/

Biogeosciences
Discussions
Open Access

Interactive comment on "Pollen-based paleoenvironmental and paleoclimatic change at Lake Ohrid (SE Europe) during the past 500 ka" *by* L. Sadori et al.

L. Sadori et al.

alessia.masi@uniroma1.it

Received and published: 27 November 2015

First of all we would like to thank Chronis Tzedakis for the positive comments and constructive remarks on our manuscript.

1. A great strength of the record is that it is supported by a detailed chronological framework. While the derivation of the timescale is presented at another paper by Francke et al. (this volume), it is important to provide a more detailed description of this here, in order for the present paper to be able to stand alone. A figure showing the position of the different types of control points is therefore required. Having the read the Francke et al. paper, I have the following comments to make on the derived timescale. While the

1st-order control points are derived from the chemical fingerprinting of tephra layers to known eruptions providing an independent chronology that is extremely valuable , the 2nd and 3rd order controls are derived by tuning to orbital parameters and the LR04 benthic stack, which is itself tuned to orbital changes and as such are not independent. With respect to the 2nd-order tuning, minima in TOC content and in the TOC/TN ratio have been aligned with inflection points of increasing summer insolation on 21 June at 41° N, as suggested by the position of some tephra layers. An explanation involving a balance between summer insolation strength and winter season length leading to low organic matter preservation is presented, which may or may not be correct. In fact, the control points of the 2nd-order are placed at times when perihelion passage occurs in March and it is worth recalling that Magri Tzedakis (2000, QI), Tzedakis et al. (2003, EPSL) and Tzedakis et al. (2006, QSR) already noted that tree population crashes corresponding to dry and cold episodes occurred at times of perihelion passage occurring in March. Berger et al. (1981) have pointed out that the highest radiative loss through surface albedo in middle and high latitudes occurs in spring. Thus, relative minima in shortwave absorption would occur during intervals when a large part of the annual radiation is delivered at this time (i.e., March perihelion configuration), and this pattern could provide a mechanism for the observed periodic coolings and related impact on ecosystems. As for the 3rd-order points, aligning terrestrial records to the benthic isotopic stack may have once been considered broadly sufficient (e.g. Tzedakis et al. 1997, EPSL), but is no longer the optimum way for constructing a detailed chronology as more recent work on pollen sequences from deep-sea records has shown that benthic $\delta^{18}O$ and terrestrial events are not necessarily coeval (e.g. Shackleton et al., 2002 QR; Tzedakis et al., 2004 Science). The climatic explanation for aligning the TIC to the LR04 stack is not tenable on two additional grounds: (i) benthic records contain a signal of changes in the isotopic composition of seawater, deep-water temperarure and hydrographic effects and unless these are deconvolved, it is not possible to interpret changes in terms of ice volume only (e.g. Elderfield et al., 2012 Science); and more crucially (ii) even if the ice volume component were isolated, the different response times of ice sheets, ITCZ shifts and local climate means that a simple alignment between the ice volume and TIC is not straightforward. I would therefore recommend that these control points be removed, or replaced by control points derived from alignment with $\delta^{18}O$ records from Mediterranean planktonic foraminifera or sea-surface temperature records, which have been shown to be more in-phase with terrestrial climates. The problem with this, however, is that then once cannot make comparisons with the same records (as in section 4.2). I realize that the authors of this paper were not involved in the derivation of the chronology, but since this is part of a large collaborative project the possibility of making revisions to it should be discussed with Francke et al.

Answer: We appreciate very much the suggestions on improving the age model of the Lake Ohrid record. The age model is dealt in Francke et al. (this issue) and is beyond the scope of our study. We would like to note, however, that our revised manuscript will be based on a new revised chronology that has been developed by Francke et al. and by Just et al., following the suggestions by C. Tzedakis and other reviewers. In order to achieve maximum clarity for the readers we will include more information on the age model in our revised manuscript, including the most important tie points.

2. While hirtherto, pollen records (including long sequences) were usually produced by a single researcher, or a couple of researchers from the same laboratory, the palynological investigation of Ohrid provides a glimpse of the future of long sequences, where the laborious analyses are undertaken by several investigators across many laboratories, as part of an international collaborative effort. As this is probably the first undertaking at such a scale, it would be very useful to know how the partners ensured comparability in pollen identification and whether an attempt at ensuring reproducibility of results was made (e.g. interlaboratory comparison of preparing and counting samples from the same depths).

Answer: Prior to the pollen analysis, considerable time has been invested in in assessing and standardising the treatment protocol and pollen identification issues. More specifically: (1) we have joined previous lists of taxa that were derived from older studies in Lake Ohrid and western Balkans and produced a final list that has been accepted by all the analysts; (2) we have thoroughly elaborated on systematic issues like synonyms and different degrees of pollen determination, particularly focusing on the identification of problematic taxa; (3) we shared pollen pictures of key-taxa (e.g. oak types) and of dubious ones via dropbox; (4) we have also performed analysis of samples from the same core depth in different laboratories. Samples have been distributed in batches of consecutive samples; (5) finally, close checks have been performed at the intervals where two different analysts' samples met in order to avoid any potential identification bias. A short paper dealing also with these issues related to palynological protocols and analyses will be soon submitted to Alpine and Mediterranean Quaternary (Bertini et al.).

3. The pollen concentrations, as is usually the case, are characterized by orders of magnitude changes and the authors have opted to present them on a logarithmic scale. However, that tends to obscure the extreme values, which are often of most interest. In fact, the zone with the lowest arboreal pollen concentrations is OD-11 (second part of MIS 12), which is in line with the canonical view of MIS 12 being the most extreme glacial of the last 500 kyr, if not of the Quaternary. The authors say that the presence of high values of *Pinus* indicates that the climate was wetter than other glacial phases, but this may be misleading, as it could arise from taphonomic issue (see next point). On the other hand, it is true that the OD-11 (late MIS 12) is dominated by grasses with relatively low Artemisia and chenopod values, which could suggest higher moisture availability than later glacials (MIS 6 and MIS 4-2). However, this remains counterintuitive, because both arboreal and non-arboreal pollen concentrations are the lowest of all glacials. Maybe late MIS 12 was extremely cold, but not very dry as the authors suggest, but I can't quite envisage the climate setting that would lead to extreme cold but not extreme aridity at the time of the largest Pleistocene ice sheets. Do the sediment analyses provide any indication of a possible hiatus? Until this is clear, perhaps one might avoid drawing any climatic inferences in this part of the record.

Answer: To comply with the reviewer's suggestion, we will add the concentration curve in normal scale and also use these data to improve our interpretation. We fully agree with the reviewer that the interpretation of climate is complicated as grasses could come either from a lacustrine belt or from grassland formations in the catchment of Lake Ohrid. Our interpretation of increased humidity is also supported by the higher abundances of montane forest pollen and low pollen abundances of pioneer taxa between OD-10 and OD-13. A likely explanation of increased humidity, and consistent with the high endemism and biodiversity of the site possibly due to the buffering capacity of the lake, is the fact that a part of pine pollen could be from *Pinus peuce*, which is adapted to cold and moist conditions and currently has only a relict distribution. The relatively low abundance of xerophytic Mediterranean "ecogroup" also supports this view.

4. Perhaps the most substantive comment concerns the interpretation of the *Pinus* values. The authors have justifiably removed *Pinus* from the pollen sum, due to its overrepresentation. However, this overrepresentation appears to be more extreme in the lower part of the core (below 145 m) and especially during MIS 12 and MIS 10. In some respects this is reminiscent of the overrepresentation of *Pinus* in marine cores (e.g. Portuguese Margin), where the values of pine are higher in the glacials. This may be related to the low arboreal pollen concentrations during those periods and/or a change in the depositional setting through time. Is it possible that there was a change in lake basin size after 330 ka?

Answer: To our understanding, the reviewer puts forward the hypothesis of a bigger and deeper lake in order to explain the selective transport of pine pollen to the coring site considering that pine pollen float easily on the water surface. however, the available seismic data, not completely processed yet, suggest (K. Lindhorst and S. Krastel, personal comments) that the lake size was not significantly different prior to 330 ka (which is in concert with the shape of the lake that is characterized by relatively steep slopes to the east and west of the coring location. If the lake level was significantly lower in the past, the effect should have been stronger at the south and north parts of the lake, but it would not affect the minimum distance to the shores east and west of the coring location. Given the uncertainties on the origin of the high pollen percentages of *Pinus* - often exceeding 95 % in some samples from MIS 10 and MIS 12, we decided to remove *Pinus* from the total pollen sum. The high *Pinus* percentages may be partly related to taphonomical issues given that the pollen of this taxon are relatively more resistant. But this preservation effect does not explain the observed changes in *Pinus* abundance across glacial-interglacial cycles in the lower part of the record.

5. Throughout the text, the authors refer interglacial complexes of MIS 5, MIS 7 and MIS 9 and MIS 11 as "interglacials". This is not correct, because only MIS 5e, 7e (and also 7c), 9e and 11c are of interglacial status, the others are interstadials, with residual ice volume outside Greenland. 6. I am not sure that the comparison with benthic isotopic records (LR04 or the Med stack) provides any insights. By contrast, the comparison with the planktonic isotope record from the Mediterranean provides more opportunities to discuss the similarities in greater depth.

Answer: We fully agree with the reviewers and we will revise the text accordingly when referring to interglacials or interstadials. In addition, we will attempt a more detailed comparison of the pollen data with the Mediterranean planktonic isotope record.

Minor comments Page15464, line 1: replace "first" with "earlier" P15464, l. 17: replace "60ies" with "1960s" P15465, l. 1: "Martrat et al., 2007" did not really attempt marine-terrestrial comparisons. I would instead use: Tzedakis, P. C., Roucoux, K. H., de Abreu, L. Shackleton, N. J. (2004) The duration of forest stages in southern Europe and interglacial climate variability. Science 306, 2231-2235 p. 15473, l. 2: There is also a study of MIS 7 from Ioannina: Roucoux, K.H., Tzedakis, P.C., Frogley, M.R., Lawson, I.T. R.C. Preece. (2008) Vegetation history of the marine isotope stage 7 interglacial complex at Ioannina, NW Greece. Quaternary Science Reviews 27, 1378-1395. p.15474, l. 9: It is not entirely correct to say that the duration of "glacial conditions" C6528 BGD 12, C6524–C6529, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper was longer, one can only say that the duration of "non-forested periods" at Ohrid was longer. p.15477-8: A comparison of the climatic and vegetation character of Ioannina, Kopais and Tenaghi Philippon was presented in: Tzedakis, P.C., Frogley, M.R., Lawson, I.T., Preece, R.C., Cacho, I. de Abreu, L. (2004) Ecological thresholds and patterns of millennial-scale climate variability: The response of vegetation in Greece during the last glacial period. Geology 32, 109-112. Finally, a minor problem (but one that can lead to future complications) is the numbering of the zones and their hierarchical classification. When high-resolution analyses will be undertaken, this will necessitate the definitions of more pollen zones. If these are then given sub-zone status, you can end up having a biostratigraphical subzone corresponding to a chronostratigraphical stage (e.g. the Last Interglacial), instead of corresponding to a (sub) chronozone (and would further zones within that stage, correspond to sub-subzones?). One way around this is to define superzones corresponding to stages now and that will allow a hierarchical classification system (e.g. Tzedakis, 1994, JQS 9, 257-259).

Answer: We will work on these minor/technical suggestions in our revised manuscript.

Interactive comment on Biogeosciences Discuss., 12, 15461, 2015.