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Discussions

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

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It is a source of great pleasure to see the emergence of a long pollen sequence that will provide a whole range of insights into long-term climate and vegetation dynamics on a variety of timesclales for years to come. The MS by Sadori et al. represents the fruits of an international collaboration to retrieve and analyse a long sediment record from the oldest extant lake in Europe, Lake Ohrid. It presents the top 200m ($\sim$500 kyr) of a 569m-long composite sequence, at $\sim$ 1.6-kyr resolution, supported by detailed lithological and chronological analyses that are being presented in accompanying papers in the same issue of BGD. Although the record presented here is of preliminary nature

(the authors describe the pollen diagram as skeletal or overview, with higher-resolution analyses expected to emerge in the future), it is comparable to the resolution of many existing pollen records and perfectly adequate to draw some preliminary conclusions about first-order glacial-integlacial changes and trends.

The paper is very well written, carefully argued and well illustrated. I have some general comments to make, followed by some minor points. These are not meant to be a criticism of the work, but rather suggestions for improvement, which I hope the authors will consider. As such, I recommend publication with minor corrections.

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 indepencent.

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 d18O 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 d18O 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.

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 countingsamples from the same depths).

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.

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?

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.

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"

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).

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