

Reviewer #2 (W. Fletcher)

General comments:

The manuscript “Climate variability since MIS 5 in SW Balkans inferred from multiproxy analysis of Lake Prespa sediments” by Panagiotopoulos et al. contains the findings and interpretation of new multiproxy investigations of lake sediments from the Balkan Peninsula. The manuscript contains a wealth of original palynological data, accompanied by geophysical and geochemical proxies, that provides indications of local and regional environmental change during the last 90 ka. The study is based on a sound methodology, and is supported by a detailed chronology that has been previously published in *Climate of the Past* (Damaschke et al., 2013, *Clim. Past.* 9:267-287). The study examines local vegetation and limnological changes in considerable detail. These in general support the prevailing views of environmental and climatic change for the last glacial. Nevertheless, the study represents an important contribution to the growing understanding of spatial variability in environmental records of the last glacial from southern Europe. The manuscript is clearly written and supported by relevant, high-quality figures. I would recommend publication, taking into consideration the following comments:

[We thank W. Fletcher for his thorough review that improved our paper.](#)

Specific comments:

Section 1. The authors suggest that the value of the new record relates to “*the limited number of glacial records originating from the Balkans in comparison with the Italian and Iberian peninsulas*”. I don’t entirely agree with this, as records spanning the full glacial are not really abundant anywhere, and one could argue that the Greek records (e.g. Ioannina, Kopais, Tenaghi Philippon, Xinias) constitute one of the greatest densities of long pollen records anywhere in the world. In fact, one of the key messages of the manuscript (presented, for example, in the final lines of section 5.3 and figure 6) is the interest in developing a “dense network” of sites to examine intra-regional patterns of environmental and climatic conditions. I would recommend that the authors modify the introduction to prioritise this valuable point.

[We acknowledge that the introduction \(section 1\) could highlight the advantages of working in an area with relative abundant long pollen sequences \(southern Balkans\) instead of stressing the lack of glacial records in northern Balkans and in particular outside Greece. We modified the second paragraph on p. 1323 accordingly.](#)

Section 2. Although the vegetation has been described in detail elsewhere, I would encourage the authors nevertheless to include 1-2 sentences here to indicate the dominant vegetation cover in the vicinity of the site, so as to provide some context for the vegetation reconstruction. Mentioning the present-day role of *Quercus* and *Pinus* species would be beneficial, for example.

We added the following sentence on p. 1325 line 21:

“(Polunin, 1980). The major vegetational formations encountered at Prespa in descending order are the alpine and subalpine meadows, the montane conifer forests (notably *Pinus peuce*), the montane deciduous forests (dominated by *Fagus sylvatica*), the mixed deciduous oak forests (with thermophilous species closer to the lake) and the grasslands of the littoral zone.

Section 4.2. Paragraphs 2 and 3 of this entire section could be better presented in a table. The text would flow naturally into section 5.1, and it would be easier to cross-reference between the key data and the interpretations in later sections.

We removed the last two paragraphs from Section 4.2 (describing the PAZs) placing them in an overview table (Table 1) instead. Table 1 will contain a brief description of the results (mostly pollen, geochemistry, lithology) as well as a short interpretation (keywords) of the inferred paleoenvironment.

Section 5.1.1. I'm not convinced that the title of this section accurately reflects the content, particularly the concept of feedbacks. “Vegetational and limnological feedbacks to climate variability at a local scale” suggests that the text will explore how local environmental change amplified or dampened climate signals through biophysical or biogeochemical feedbacks. I think the title should read “responses to climate variability”.

We changed the title of Section 5.1.1 to “Vegetational and limnological responses to climate variability through space and time”. In this section (5.1.1), the meaning of our biotic and abiotic proxies is discussed in greater detail.

P1332. Line 8. The authors could show the curve for AP concentration in Figure 5 to support the argument here.

The AP concentration curve is already cited in Line 1 P1332. For clarification we recited it in Line 8(Fig. 5d).

Section 5.1.2. This section is quite long, very descriptive and focused entirely on the findings from the site; as such I'm not convinced that it's really “discussion” as such. I would be interested to see the section either supported by references to the wider literature on glacial environments, or condensed.

Considering the remarks of the anonymous reviewer, we decided to shorten the descriptive parts of the paper and thus we altered the organization and structure of the manuscript. Section 5.1.2 is removed; the more descriptive parts of 5.1.2 are incorporated in Table 1, while proxy interpretation and discussion is inserted into Sect. 5.1.1, which is now named “Vegetational and limnological responses to climate variability through space and time”.

Section 5.2.1. The detailed comparisons with Monticchio and Ioannina are clearly justified in terms of altitudinal/ecological setting, but I think this section could be enhanced by bringing other key regional glacial records into the discussion, such as the high-resolution vegetation record from Tenaghi Philippon (Müller et al., 2011) and the speleothem record of Fleitmann et al. (2009). What can be learned about intra-regional spatial gradients or temporal trends and variability?

The pollen record of Tenaghi Philippon by Müller et al. (2011) is already cited in our paper (see section 5.3) in context of modern human migration. As the reviewer suggests, a comparison could be interesting, however, the TP site does not meet the criteria (elevation, submediterranean vegetation etc.) defined in the first paragraph of section 5.2.1 (p.1342). We decided to use the speleothem record from Israel (Bar-Matthews et al., 2000) over the one from Turkey (Fleitmann et al., 2009) as a reference archive as it covers the entire period examined in our paper without significant discontinuities and is located upon the major migration path of modern humans out of Africa. We also have a great interest in spatial ecological gradients and time lag questions as the reviewer points out in his closing phrase and therefore we are planning to increase the temporal resolution of the Prespa pollen record in particular within the MIS 4-2 intervals. A more detailed discussion over temporal and spatial patterns is in our judgment at this moment premature given the sampling and dating constraints already discussed.

Section 5.3 (especially lines 15-19) The authors correctly note that the severity of H5 in the record is subject to dating and sampling constraints – and this caveat might be stressed earlier in the manuscript, too (cf. Section 5.2.2.). An average sampling resolution of 500yr will detect millennial-scale variability, but will not provide a systematic detection of all events or robust characterisation of amplitude (i.e. maximum and minimum values).

We modified the text in section 5.2.2 accordingly.

Corrections/Clarifications:

P1323. Line 5. Spelling “Oeschger”
Corrected.

P1325. Line 6. It’s not clear what “the annual lake level change” refers to. . . this hasn’t been specified.
Apart from annual lake level changes, some seasonal fluctuations (up to 1m) are recorded at Lake Prespa.

P 1343. Line 15. “values in specific in” – change to “values, in particular in”
Modified.

P1347 Line 13. Replace “invaded” (value-laden term) by “entered” (neutral).

Corrected.

P1348. Line 1. Change “self” to “shelf”

Corrected.

P1348. Line 2-3. Can a reference be given to support the use of continental shelf areas for dispersal routes? If not, it's not clear that the phrase “In most certainty” is warranted, and perhaps should be changed to “It is possible. . .” or similar.

Modified.

P1348. Line 6. Indicate reference for Lateglacial study here.

Added.

P1349. Line 11. Give references for “speleothem record(s)”

Added.

References:

Fleitmann, D., Cheng, H., Badertscher, S., Edwards, R.L., Mudelsee, M., Göktürk, O.M., Fankhauser, A., Pickering, R., Raible, C.C., Matter, A., Kramers, J., and Tüysüz, O., 2009, Timing and climatic impact of Greenland interstadials recorded in stalagmites from northern Turkey: *Geophys. Res. Lett.*, v. 36, p. L19707.

[Please see answer to section 5.2.1.](#)

Müller, U.C., Pross, J., Tzedakis, P.C., Gamble, C., Kotthoff, U., Schmiedl, G., Wulf, S., and Christanis, K., 2011, The role of climate in the spread of modern humans into Europe: *Quaternary Science Reviews*, v. 30, p. 273-279.

[This reference already exists \(p1354 line 5\)](#)