

Interactive comment on “Climate variability since MIS 5 in SW Balkans inferred from multiproxy analysis of Lake Prespa sediments” by K. Panagiotopoulos et al.

Anonymous Referee #1

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General comments

Panagiotopoulos et al. present in this paper a –partly- original dataset from a 18m-long core taken in Lake Prespa (Balkan Peninsula) and covering the last 92 ka. The presented dataset comprises pollen analysis, as well as some geochemical (TOC, TIC and C/N punctual measurements; Ti and Fe content from XRF core scanner) and mineralogical (occurrence of siderite) data. It is partly original as several previous papers form the same team dealt with the Late Lateglacial-Holocene period using various proxies and the last 50 ka TIC and TOC data (as well as core-scanner-derived Mn data) were published by Wagner et al. (2010, Biogeosciences). However, due to the rareness

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of such long continuous paleorecords and the amount of additional work compared to previous papers, one can consider the dataset as novel and of scientific relevance.

My main criticism about this manuscript regards its organisation. Indeed it has been very hard for me to catch the pitch of it, and finally I must admit I didn't. As a non-specialist of those periods and pollen data, I have been lost in pages of redundant description of paleoenvironmental interpretation of pollen data. I would recommend the author to discuss first the meaning of their proxies and thereafter describe the evolution of them through time instead of discussing the interpretation of the same proxy several times for each considered time-period.

Moreover, I feel like the title does not reflect the real content of the paper as no real climate proxy is presented here as a main result. The discussion about climate is hence not based on presented original data which do not bring important novel perspectives on it. In order to better extract the climate meaning of the dataset, why not using specific indicators such as PCA scores representing the vegetation response to temperature, drought etc.? However it seems to me the great interest of the paper is not the bringing of new paleoclimate data, but valuable information about how ecosystems did change and adapt throughout the major climate changes of the last glacial period. I would recommend enhancing this point rather than arguing this record is really a new paleoclimatic one.

The same is true regarding the discussion about human dispersion to Europe in which Lake Prespa data are virtually absent. Despite the major scientific interest of this issue, I would recommend the authors not to deal with in this paper of which it is not the point: this makes the paper even more confusing.

Overall, despite the quality of the presented dataset, I would not recommend Climate of the Past to publish this paper as it stands. I would recommend the authors to think about the main message they want the reader to keep with him while reading this paper and to adapt their manuscript in a more demonstrative way prior to resubmit it.

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Specific comments

My main specific comments regard geochemical and mineralogical data. I wonder why the authors use the Fe/Ti ratio as a proxy of redox conditions whereas Wagner et al. (2010, Biogeosciences) showed Mn content is a good marker for more oxygenated periods?

The whole reasoning about carbonate precipitation is hard to follow. In particular the authors never evocate the effect of temperature on carbonate solubility which, in the cases I know, is often the main driver for carbonate precipitation at the glacial-Interglacial time-scale. Despite the question of temperature, I wonder in what extent the degree of soil development would not be a driver for carbonate solubility as it can in one hand increase the dissolved carbonate content of water and in the other hand release humic acids that tend to increase the carbonate solubility in lake waters. Could the authors discuss this point?

Technical corrections

The description of C and N measurement methods is really poor. In particular it is not clear if the C/N was computed from the total carbon or only organic carbon.

Figures are relatively few (only 6) regarding the density of the paper and the amount of discussed data. They are very hard to read (font is definitely too small).

Interactive comment on Clim. Past Discuss., 9, 1321, 2013.

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