Reviewer #1 (Anonymous)

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 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.

We thank the anonymous reviewer for taking the time to review our manuscript. The core (Co1204) discussed in Wagner et al. (2010) was retrieved from a lateral part of the lake and is apparently suffering from a hiatus during the Lateglacial transition. Core Co1215 (1776 cm) presented here is the first core from Lake Prespa that has been palynologically

analyzed. As it is clearly stated in Section 4 (Results) the paleovegetation and paleoenvironment at Prespa during the last 17,000 (from the upper 320 cm of Co1215) years were discussed in detail in Aufgebauer et al. (2012) and Panagiotopoulos et al. (2013), while Wagner et al. (2012) and Leng et al. (2013) presented sedimentological, hydrological and geochemical data for the upper 1576 cm. This paper wraps up sedimentological and geochemical data (1776 cm) and with a focus on new biological proxies and ecological processes aims at understanding the complex (abiotic and biotic) responses of the Lake Prespa catchment to climate variability during the last 92 ka.

We tried to present the paleovegetational results and their interpretation over several thousand years in a way that remains accessible and comprehensible to non-pollen specialists, but at the same time paying attention not to oversimplify the presented biological proxies. Exploratory statistical analyses (including ordination and rarefaction) were performed on the pollen dataset, but in our judgment their addition to the current manuscript was not considered as an indispensable contribution to the discussion. We disagree with the reviewer's argument that reconstructed paleovegetation (complemented here by sedimentology and geochemistry) does not offer novel insights into climatic conditions prevailing at Prespa (a mid-altitude site in the Balkans) over the Last Glacial. Palynology has been instrumental in inferring past climate variability at different temporal and spatial scales. Although it is an indirect climatic inference, our pollen-based qualitative reconstruction does offer original information on climate conditions at a local and regional scale for the period examined. Within the CRC 806 'Our Way to Europe', we have already developed in collaboration with colleagues from Bonn University (B3 project) a pollen-based quantitative reconstruction of climatic conditions at Prespa over the last 17,000 years that we are looking forward to publishing shortly.

Taking into consideration the remarks of both reviewers, we altered the organization and structure of the manuscript and condensed some parts. 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. 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. 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.

This study forms an integral part of the CRC 806 'Our Way to Europe' dealing with the dispersal of modern humans out of Africa and as such provides novel paleoenviromental and paleoclimatic insights from a mid-altitude site in the Balkan Peninsula located along a major migration route into Europe. We believe that long and continuous paleoarchives (such as the one from Lake

Prespa) can refine our understanding of environmental and climatic conditions that facilitated or hindered modern human mobility at a regional scale. Based on the comparison of our pollen record with other regional reference archives, we argue that the SW Balkan region was a potential refugium not only for flora, but also for fauna (including our ancestors). Owing to the diversity of proxies and scientific principles found in our discussion, we do realize that the manuscript may pose a challenge to experts with different scientific backgrounds. Nevertheless, we consider this interdisciplinary multiproxy approach as an integral strength of the manuscript and we are interested in delivering to a wider audience besides geoscientists including paleoecologists and archaeologists.

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 Fe curve (normalized against Ti) was chosen in favor of the Mn one in order to facilitate the interpretation of periods when siderite ($FeCO_3$) was precipitated. The manganese curve mirrors the iron one, which is not surprising as Mn commonly substitutes Fe. Manganese redox reactions are similar to those of Iron, except that they proceed more slowly. Reduced Mn^{2+} is oxidized to Mn^{4+} under somewhat more oxidizing conditions than Iron.

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?

We are in agreement with the reviewer; therefore, we modified this part of the discussion (see also changes in the organization of the manuscript described in our answer to the general comments). There are several environmental parameters that control the precipitation of carbonates and temperature changes played undoubtedly a major role on a glacial-interglacial scale (e.g. continuous calcite precipitation and preservation in the Holocene). We inserted the following sentence in the text (p. 1334 line 20:
"…(Holocene). Increases in temperature (or salinity) may cause the removal of CO_2 through lowered solubility and calcite precipitation. Calcite precipitation…".
Although the point of soil development as a driver for increased carbonate solubility is of relevance, it has already been discussed in detail in Leng et al.

2013 (e.g. Section 6.4). However, we added the following clarifications and citations to the paragraph dealing with carbonates on p. 1334:
"… Isotopic data ($\delta^{13}C_{org}$, $\delta^{13}C_{TIC}$) suggest that during the Glacial carbon input was limited; most likely due to more limited recharge of soil-CO_2 leached from the catchment, while in the Holocene a greater supply of soil-derived CO_2 is inferred (Leng et al., 2013). Moreover, the pollen also suggests well-developed soils during the Holocene inferred from increased forest cover and tree species diversity (Panagiotopoulos et al., 2013).

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.

A detailed description of the geochemical analyses (including the C/N analysis) can be found elsewhere (Aufgebauer et al., 2012 and Leng et al., 2013) as it is clearly stated in Section 3.1 (Geophysical and geochical analyses).
For clarification the following two sentences were entered at Line 15 p. 1326:
"…from TC. Concentrations of total carbon (TC) and total nitrogen (TN) were measured with a Vario Micro Cube combustion CNS elemental analyzer (VARIO Co.). The atomic TOC/TN ratio (abbreviated as C/N) was quantified in order to identify the source of the organic matter in the lake sediments (cf. Meyers and Ishiwatari, 1995). The identification…".

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).

We paid special attention to the legibility and details of our figures presenting only data that are relevant to the proxies presented and discussed in the manuscript. The font sizes and artwork comply with the Climate of the Past requirements for illustrations. Perhaps a possible source of confusion is the landscape (A4) format of Clim. Past Discuss., which makes the figures appear much smaller than the original ones (all three diagrams Fig.4-6 should appear as full page figures in profile orientation).