Referee #1

This paper presents a significant body of new information on Lake Ohrid and its environmental history. I strongly support publication of this work. I look forward to a more careful discussion of iron in these sediments (Fe, Siderite variability) as part of the paleomagnetic chronostratigraphy and potential for early Fe sediment diagenesis.

We would like to thank referee #1 for the very positive review and we much appreciate his interest in the occurrence of diagenetic iron minerals. In order to meet the comment of referee #1, we will include some sentences with respect to the presence of Fe-sulfides, which is discussed in more detail in the recent paper of Just et al. (2016).
From the FTIRS and magnetic data it is evident that a shift of (early) diagenetic Fe-sulfides to siderite occurs around 320 ka. Notably these minerals predominantly occur in glacial sediments. Just et al. (2016) proposed that a change in sulfide availability, either by higher sulfate concentration in lake water or by upward migrating fluids, changed the geochemical regime in Lake Ohrid. Moreover, the occurrence of the siderites and Fe-sulfides in glacial sediments is likely related to higher Fe concentrations.
The, yet unpublished, data from the lower part of the core, imply that the glacial-interglacial variability of Fe-sulfides persists down to the base of the core. In consequence, polarity transitions are clear when they are located in interglacials, i.e., the Bruhnes/Matuyama and the base of the Jaramillo are very sharp. In contrast, the top of the Jaramillo is uncertain, and possibly duplicated by a later growth of greigite below the sediment surface. Analytical work is still ongoing and the paleomagnetic chronostratigraphy will be used along with tephrostratigraphic information and orbital tuning to establish an age model for the lower part of the sediment record, i.e. below 247.8 m composite depth or beyond ~640 ka. Moreover, we are currently performing sulfur isotope analyses on Lake Ohrid sediments, which will help to understand the change and source of sulfide concentrations. The age model for the entire DEEP site record and the results of the sulfur isotope analyses will be published in the near future in a separate paper.

Referee #2

The MS presents a synthesis of initial results of the SCOPSCO deep drilling of Lake Ohrid project, previously published in a series of papers in Biogeosciences. It brings together information from the four main aims of the project (age and origin of lake; seismotectonic history; volcanic activity and climate change; biodiversity and endemism) and compares results from different types evidence and approaches. As such, the whole is greater than the sum of its parts and the study is of great value to the scientific community. The text is well-written and organized and the figures of excellent quality. I have one substantive comment and one minor quibble.

We also would like to thank referee #2 for the very positive review and valuable suggestions.

1. A potentially important conclusion emerging from several strands of evidence is a long-term trend from cooler and wetter to drier and warmer glacials and interglacials, starting at ~300 ka. However, closer examination reveals that the trends between different types of evidence are not always congruous.

Water depths estimated from seismic data suggest a decrease in lake levels from 300 ka, but the trend is reversed from MIS 4 to today, with water depths increasing. The authors suggest (p. 17, l. 22) that this is in broad agreement with regional vegetation trends inferred from pollen analysis, but this not entirely accurate: the pollen data show that the two driest periods were the penultimate (MIS 6) and last glacial (MIS 4-2). This is mainly based on the large expansions of Artemisia during these *intervals, vis-à-vis very low values in earlier glacials (incidentally, a feature that has not been observed in other long pollen sequences).*

The claim that pollen data and inferred water depths show parallel trends is repeated on p. 20, but, again, if water depths increase from MIS 4, then there is divergence between the two over this interval. The pollen data suggest that in addition to glacials, a drying trend is also observed in interglacials. This is mainly based on the reduction of montane tree values in MIS 5 and MIS 1 (especially the almost complete disappearance of Picea; though Fagus increases somewhat from MIS 5c onwards) (Sadori et al., 2016). On the other hand, Mediterranean taxa percentages don't show any trend apart from a brief maximum at the MIS 4/3 boundary (which is unexpected), so it might be more useful to show the montane taxa in Fig. 4.

The drying trend theme is picked up again on p. 21, with the oxygen isotopic evidence. More specifically, a trend towards higher interglacial d18O in endogenic calcite after 300 ka is invoked, but close inspection shows that it is only MIS 5 that shows that; Holocene values are not that different from earlier interglacials. Interestingly, the d18O record from siderite shows lowest values in the penultimate and last glacials. This is interpreted (p. 21, 30-31) as evidence for lower evaporation during glacials, which is reasonable. However, it is also interpreted as a "higher influence of winter precipitation (increased seasonality), which supports the interpretation of the palynological record". This, in fact, appears at odds with the high Artemisia expansions.

In conclusion, while the inference of a drying trend is potentially a very interesting and exciting observation, I would suggest that a more nuanced interpretation is needed, as close inspection reveals a more complicated picture amongst the different lines of evidence.

We completely agree with the referee that the inference of a drying trend with cooler and wetter to drier and warmer glacials and interglacials starting at ~300 ka is a very general statement and a more nuanced discussion with respect to the individual lines of evidence will help provide the nuance. A very detailed look at each proxy including comparisons is available in the individual papers, particularly in Lacey et al. (2016) and Sadori et al. (2016). As this new paper here is designed as an overview paper, we will try to keep the discussion more general, but will add relevant information to make differences more clearly.

- We stated already in the text that the hydro-acoustic data do not allow us to infer detailed and timely well-constrained lake level or climate changes due to tectonic activity and chronological uncertainties. This is evident as the new reconstruction supposes lowest lake level during MIS 5, whereas hydro-acoustic and sediment core data in Lindhorst et al. (2010) infer lowest lake level during early MIS 6. We will re-check the text, if this needs to be pointed out more clearly.

- The pollen data show an unexpected behavior of *Artemisia*. High amounts of other herbs (grasses, chenopods, Cichorioideae and Cyperaceae) are found already in MIS 10/12 glacials and indicate the presence of open formations, even if less dry than those characterized by *Artemisia*. The remarkably high amount of *Pinus* pollen grains in specific intervals (i.e. MIS 9 to 12) of the skeleton DEEP Lake Ohrid pollen diagram was already identified as an issue in Sadori et al. (2016), which led to the decision to exclude pines from the pollen sum. We are confident that the basic signal produced is reliable, but we will carefully cross-check sample processing. The low *Fagus* percentages are described in more detail in Sadori et al. (2016) and we agree with referee #2 that there is a slight increase in *Fagus*, when spruce decreases, which is probably the result of a rearrangement of vegetation in altitudinal belts.
We also figured out that there was an error in the drawing of the Mediterranean taxa and the pioneers curve. We will submit a corrigendum to the Sadori et al. (2016) paper. As the short peak of Mediterranean taxa at the MIS 4/3 boundary resulted from the error in the drawing

and this curve is not so significant for a site such Ohrid, we will remove this curve from Fig. 4 and use the corrected drawing (see below).

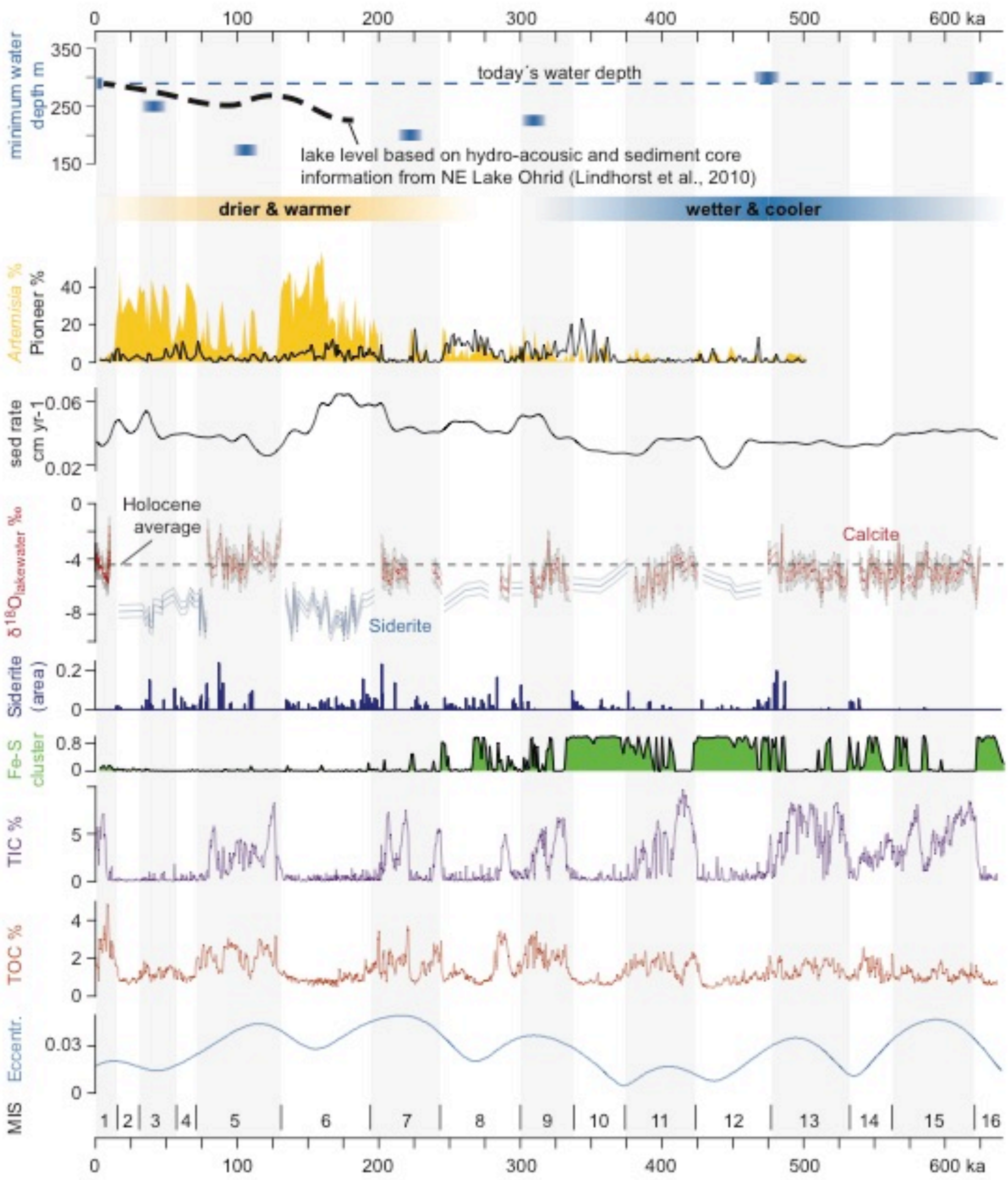

Figure 4 of the BGD paper, modified according to the discussion in the text.

- We do not agree with the comment that only MIS 5 shows a drying trend. Lacey et al. (2016) show a comparison figure of average d18O values, which clearly shows MIS 5 and 1 have the highest average baseline of the last 640 ka. Also, when calcite data or "warm periods" (rather than inter glacials senso stricto) are considered, there is a clear drying trend through from 300 ka after the transition to lower d18O in MIS 9. The isotope data thus support the very general lake level reconstruction based on the hydro-acoustic data with lowest lake levels during MIS 6 or 5. We agree with referee #2, however, that the sentence "higher influence of winter precipitation (increased seasonality), which supports the interpretation of the palynological record" needs to be removed, as the two proxies do not show an unequivocal pattern of seasonality, thus confirming the need for a more nuanced interpretation and discussion.

2. Referencing appears somewhat idiosyncratic at times, with an overall tendency to cite recent works. Thus, on p. 23 the attribution for the work on D/O and Heinrich events of the last glacial should include the original papers by Bond et al. (1992, 1993, Nature) and Dansgaard et al. (1993 Nature), while for older glacials McManus et al. (1999, Science), Raymo et al. (1998, Nature) and Barker et al. (2011, Science), probably deserve a mention. On the same page, (l. 12-13), important papers on the impact of HE and D/O events include Shackleton et al. (2000 Paleoceanography), Roucoux et al. (2001, QR), Margari et al. (2010 Nature Geoscience).

While the need to limit the overall number of references in a work of this wide scope is understandable, the paucity of references on the body of work on the environmental impacts of North Atlantic millennial-scale variability in the Balkans seems to be an oversight (e.g. Tzedakis et al., 2002, Science, 2004, Geology; Margari et al., 2009 QSR; Müller et al., 2011 QSR; Roucoux et al., 2011 JQS; Fletcher et al., 2013 QSR). Finally, on the fascinating topic of the reservoir vs cradle function of Lake Ohrid, it might be worth recalling that that local buffering from extreme environmental effects in refugial areas may have not only led to reduced extinction rates, but also allowed lineage divergence to proceed, and thus refugia may have acted both as 'museums' for the conservation of diversity and as 'cradles' for the production of new diversity (Tzedakis et al., 2002 Science; Tzedakis, 2011 J. of Biogeogr.).

We tried to keep the number of references reasonable and therefore did not include important papers, which also deserve to be cited in the text. Following the suggestion of referee #2, we will add relevant papers and information.

p. 14, l. 5 References for Lake Ioannina?

Lindhorst et al. (2015) refer to Tzedakis (1994). We will add this reference in the text.

MPT: At several places mention is made of the 'end of the MPT'. Could this be more specific?

For the MPT, we will refer in the text to the period between 1250 and 700 ka according to Clark et al. (2006).

In sum, this is an extremely useful work and I am happy to recommend publication, subject to minor revision, which is needed to address the issues raised above.

Thank you !

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