Response to the review of the discussion paper by Cvetkoska et al., (2015)

We thank both reviewers, Dr. Jane Reed (Ref.1) and the Anonymous Referee (Ref.2) for their in depth assessment of our discussion paper. The comments and suggestions by both referees will definitely help us to provide an improved revised version of the paper. Since both referees expressed similar concerns mainly regarding the definition of the concepts and interpretation of the diatom data, we address the general comments by both referees in this response. The responses to their short comments are presented separately.

Reply to general comments

1) Both referees agree that this paper is unique by its focus on comparing the interconnectivity of lakes Ohrid and Prespa and the assessment of the ecosystems panarchy, resistance and resilience. Though, a more in-depth explanation of the various theoretical terms used in the text is suggested.

The definitions and concepts used in the interpretation will be clarified more precisely with additional explanations for the specific terms, like: adaptive cycle, ecological collapse, ecosystem regime. However, here we would like to address several points:

- *Dr. J. Reed commented: "A central tenet of the theory (Allen et al., 2014) is that change can be driven from the bottom up, internally. It may be that they consider the influence of inflow from Prespa as internal to Ohrid but it is not clearly explained. If panarchy is to be the central theory, it would be helpful to justify more clearly the use of various theoretical terms such as adaptive cycles and ecological collapse"*

In their paper, Allen et al., (2014) state: "A complex system such as an ecosystem can be decomposed into structural and process elements that can be defined over a fixed range of spatial and temporal scales" and recognize that both, top-down and bottom-up processes control the panarchy of the ecosystem. Further on, Allen et al., (2014) recognize the cross-scale linkages as critical to the system structure and discuss the occurrence and types of regimes and regime changes within specific scale ranges.

In our discussion, we interpret some of the regime shifts within the ecosystems scale (e.g. lakes Ohrid and Prespa) and describe the adaptive cycles on the temporal and spatial scale of each of the lakes to assess the cross-scale linkages (e.g. the interconnectivity between the lakes). The adaptive capacity of the ecosystem is intrinsically related to its resilience and identifying the scales present in a system is important for assessing resilience (Allen et al., 2005; Allen and Holling, 2008). Into our application of the concept of panarchy, we consider both lakes are lower scale entities of the landscape (6.2. Lines 5−7). This identification of the scales is justified by the hydrological connectivity and possible interactions between the lakes.

In the introduction (p.15053, ln 25−29) we defined the aim to analyze the temporal variations in both lakes to identify the interdependence between the lakes, as shifts in Lake Prespa might be internal drivers for variation in lake levels and productivity in Lake Ohrid. To this end, the palaeoclimate and palaeoenvironmental interpretation of the DEEP site record is thus important to identify the regimes and adaptive cycles in order to compare both records on a similar temporal scale. Therefore, we disagree with the suggestion by Referee #1: *"Thus, the palaeoclimate interpretation here is unnecessary and does not add to current understanding"*. The palaeoclimate/palaeoenvironmental interpretation will be retained in the paper, since it is essential to recognize the regimes and regime shifts which are necessary for reliable comparison between the lakes. In addition, the key points of interpretation will be reassessed according to the reviewers' comments. We will focus on the orbital-scale variability and selected short-term changes which are relevant for the comparison, like for example periods of low-lake levels and high productivity in Lake Prespa, tephra impact and the MIS 5/4 and MIS 2/1 transitions. More details will be provided regarding the concept of panarchy, resistance and resilience, as well the comparison between the lakes.

- *Referee #1 suggested: "The Lini sequence provides a higher resolution and more continuous sediment record than the DEEP"*.

We acknowledge that Zhang et al., (2015) provide more detailed diatom record and palaeoclimate interpretation of the Holocene period in Lake Ohrid. The 1008 cm sediment sequence, Co1262, studied by Zhang et al., (2015) was recovered at the western part of the lake, in front of the Lini Peninsula, comprises a 200 cm mass wasting deposit and covers the past 12.3 ka (Wagner et al., 2012; Lacey et al., 2015a). In comparison, the DEEP site sediment sequence is a continuous record and no hiatuses were identified for the time period presented in this study (Francke et al., 2015, Baumgarten at al., 2015), thus, the Lini sequence is not necessarily "more continuous". The DEEP site sequence is the main record studied for the aims of the project SCOPSCO, and the data presented here (the last ca. 92.0 ka) fit into the general aims of the project and are in higher resolution compared to previous diatom studies of core Co1202 by Reed et al., (2010) and Cvetkoska et al., (2012). Nonetheless, we will present a short interpretation of the Holocene period in our paper, since it also contributes to better understand the spatial differences in the diatom response across the lake (e.g. the Lini and DEEP site, for example).

2) Both reviewers showed concerns about the palaeoclimate interpretation in terms of adding more caution to interpreting the temperature influence. The interpretation of the diatom concentrations 'DC' as reflection of the productivity has been also questioned, with suggestion to calculate the diatom accumulation rates, 'DAR' and biovolumes. The relation of the C/N ratios is mentioned as problematic by Referee #2, as the "low C/N ratios can be indicative of planktonic dominance, either because of an increase in productivity and/or a reduction in the delivery of organic carbon from vascular vegetation source".

We thank the referees for their suggestions. Regarding the C/N ratios, we point out to the study of Lacey et al., (2015a) on the Lini sequence where it was demonstrated that in Lake Ohrid, the organic matter 'OM' is predominantly of pure algal source. The contribution of vascular vegetation to the OM at the DEEP site is also highly unlikely due to the coring location. Nonetheless, we are aware that the C/N ratios as indication for the source of OM can have several limitations. For example, the very low C/N rations (ca. 4) during the glacials can result from enhanced supply of clay-bound ammonium following mineral soil erosion rather than algal OM, along with ongoing organic carbon degradation (Holtvoeth et al., 2015). In the revised version, we will present more details in the "Diatom results and responses in the DEEP site sequence" section. In addition, we performed Spearman's rho correlation test (''stats'' version 0.8-2; R Core Team 2012) to demonstrate the correlation between the dominant diatom species, DC and the (bio)geochemistry data and calculated the diatom accumulation rates, DAR, Battarbee, 1986 (Figure 1). We will compare the DC, DAR, the dominant diatom species, and the other (bio)geochemistry proxies (the sedimentation rates, BSi, TOC, C/N, K) to provide more cautious interpretation, especially regarding the productivity.

- *Temperature-related increase in productivity:*

Based on previous studies (Stanković 1960; Allen and Ocevski, 1976), the vertical distribution of the phytoplankton in Lake Ohrid is primarily driven by temperature, in combination with light, nutrient availability, and water mixing. The diatom record of the last ca. 92.0 ka in Lake Ohrid is dominated by three species of generally different autecology, *Cyclotella fottii*, *Cyclotella ocellata* and *Cyclotella minuscula*. It has been already shown (Stanković 1960) that *C. fottii* is an oligotrophic, stenothermic species, adapted to low temperatures. Viable populations of the species have been recorded at more than 100 m water depth, where the light availability is far below the optimal intensity for photosynthesis and temperature is permanently reduced. This indicates that *C. fottii* is not limited by the low light, and temperature is more important factor for its distribution and abundance in the phytoplankton. *Cyclotella ocellata* is an ecologically successful species of wide distribution in ecosystems with various physical and chemical properties. In Lake Ohrid, *C. ocellata* dominates the epilimnetic diatom phytoplankton. This is the zone of warmer temperature, higher light availability, but lower nutrients. In lack of autecological data for *C. minuscula* we relied on the previous study of Cvetkoska et al., (2014), the findings of Winder et al., (2009) and the comparison with the geochemical proxies. At the DEEP site, the maximum peaks in the relative abundance of *C. minuscula* correspond to the Y5, Y3 and the Mercato tephra layers and to peaks in potassium (K intensities). This is also shown by the high resolution study of the diatom response to the Y5 tephra impact on lakes Ohrid and Prespa by Jovanovska et al., (2015). The tephra influx/deposition increases the silica availability, enhancing the diatom productivity, which leads to strong competition for nutrients and light resources.

In nature, the biotic response to multiple environmental factors depends on the response to the single dominant (limiting) driver, and the chance of a driver of large effect being present

increases with the number of drivers (Brennan & Collins, 2015). On a glacial-interglacial scale, multiple environmental drivers change synchronously with rising and/or declining temperatures, like the light and nutrient availability. We thank the reviewers for their suggestions regarding the temperature-related increase in productivity. Nonetheless, the species autecology, the strong correlation with the other productivity related proxies (TOC, BSi), the DC and the DAR allow to infer the trends of the response on orbital timescales as primarily driven by the large-scale changes in temperature. We will not present this as a simple linear relation, and will not exclude the influence of the other drivers, like light, nutrients, water mixing, but on a large scale consider the temperature as the dominant driver in the multiple set.

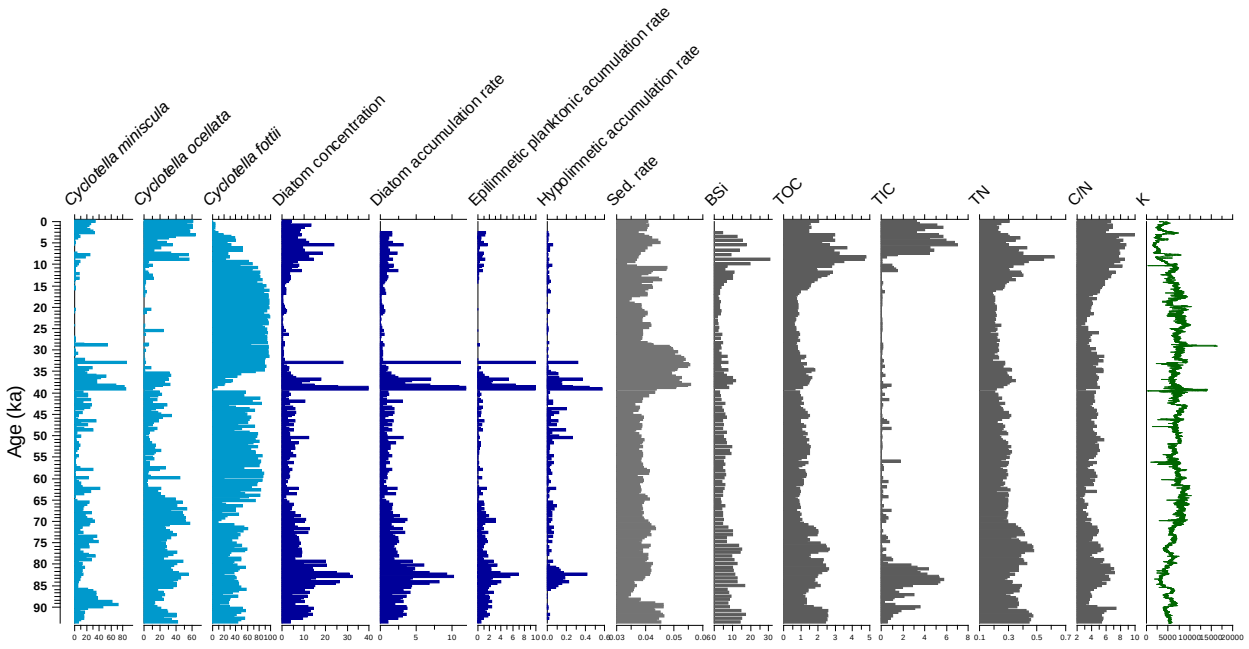

Figure 1. Stratigraphic diagram of relative abundances (%) of selected diatoms and (bio)geochemistry proxies from the DEEP site sediment sequence from Lake Ohrid. Diatom concentration data presents number of valves $*10^7 g^{-1}$. Diatom accumulation rates in valves/cm^2/ yr. Sedimentation rates in cm/yr. BSi, TOC, TIC, TN in %, K intensities as 1000 cts.

3) Lake interconnectivity: Referee #2 suggested considering the potential influence of permafrost disrupting the connectivity (and water transfer) during glacials.

We would like to thank the referee for this suggestion and acknowledge that adding the possibility of permafrost formation during the glacials will improve the discussion. We will refer to the previous inferences by Belmecheri et al., (2009). Nonetheless, Lacey et al. (2015b) already discussed the potential scenario for reduced water input from Prespa during the glacials resulting in lower lake-water δO^{18}. In addition, carbonates are not completely absent in the glacials, and carbonate (siderite) spikes have been observed in the MIS 4−2 period (Francke et al., 2015; Lacey et al., 2015b). Similar spikes during the last glacial were recorded also in Lake Prespa and

those comprise siderite (Leng et al., 2013). The palynological analyses of the last ca. 0.5 Ma in Lake Ohrid's DEEP sequence support the notion that the lake has been a refugium area for both temperate and montane trees during glacials (Sadori et al., 2015). The moraines shown in Blemecheri et al., (2009) are likely of older age then the LGM glaciation and the pollen based reconstructed LGM temperatures (T annual anomaly of -5 to -10 °C; see Peyron et al., 1998) may not cause permanent permafrost at the altitudes of Prespa and Ohrid.

4) Referee #2 suggested that more context and explanation should be added to our findings that there is no evidence that Prespa is driving changes in Lake Ohrid. The referee expressed concerns about the viability of the approach to compare the two independently dated records.

We thank the referee for the suggestion to add additional information on the models by Matzinger et al., (2006, 2007). Explanation on their empirical study (the model) will be provided in the methodology, while diatom sensitivity will be underlined in the discussion. In terms of the comparison, as the Referee #2 is also pointing out, the orbital scale changes are apparent in both records and this is where the strength of our comparison lies. Therefore, we focus our comparison on the glacial-interglacial adaptive cycles. Nonetheless, the diatom and (bio)geochemistry data shows there are no similar abrupt peaks in both records (as also pointed in the review by Referee #1), which is a good basis to compare both records and support our findings.

Reply to specific comments

Referee #1

5) The authors do present a new set of diatom and geochemical data from the Ohrid DEEP site, at higher resolution than the preliminary study of the last ca. 134 ka by Reed et al., (2010) and Cvetkoska et al., (2012), but it is a major omission to have ignored the publication by Zhang et al., (2015) of the Lini sequence.

Referee #1 criticizes ignoring their paper on several occasions throughout the review, making a clear point that we need to cite Zhang et al., (2015). We did of course not intent to leave out references to this important paper on purpose, but the paper of Cvetkoska et al., (2015) was submitted on 15 August 2015, while the paper of Zhang et al., (2015) has an on-line publication date on 1 September 2015, we were not yet aware for the status of their paper, e.g. it was not yet available to us before we submitted the manuscript. We certainly acknowledge the findings of Zhang et al., (2015) as important contribution to understanding the diatom response to the Holocene palaeoclimate variability in Lake Ohrid, and now when both discussion papers are available on-line, we will be able to compare the results and include Zhang et al., (2015) where appropriate.

- *please reword this sentence…ln 18 could read 'we compare palaeolimnological diatom and selected geochemistry data from the Lake Ohrid 'DEEP' site with equivalent data from Lake.. ln*

22 please define regime shifts. ln 23 mixing of present and past tense. ln 26 This statement needs justifying in the paper. ln 3. – see comments below. Please check English. How do you define ecological collapse? There is no discussion of feedback (positive or negative) in the paper – please delete. Should read 'may confer' rather than 'probably confine'? ln 12 Better to say continental. Also, there is no real evidence for 'evolution'….The simple term, change' might be better. Explain omega phase and justify its relevance as a concept in this study. ln 22 it is not just the intensity of external forcing. ln 24. Again, Matzinger suggests this may have occurred. ln 29 fewer. 1971 is a very old paper – is there any more recent evidence to support the statement. page 5 ln 2. Prespa is relatively shallow – much deeper than many others. lines 10, 11. Lake-level change should be hyphenated. ln 13 poor English. ln 18 core recovery. Maybe do not need to give detail of the full coring – could shorten this section to focus in more depth on the age model for the last 92 ka. ln 25 should read based ln 27 should read Lisiecki. included specifically in the Methods. If 92 ka is 37.5m in Ohrid and 17.7m in Prespa, page 7 ln 11. Wrong order of citation ln 13 it is not really imperative – reword as 'may be useful'. ln 17. ln 26 if benthic and FP taxa do not add much, maybe summarise rather than display them in the diagram. ln 28 Reed was referring to MIS2 – reword since their appearance in the Holocene is certainly not early ice out. Page 8 ln 3. – please clarify how you calculated the F index. Ln 13 Ecosystem dynamics You show good caution here in arguing that rapid change events in Prespa cannot be matched exactly with Ohrid due to uncertainty in the age model. However, if you aim is to track whether P has a major influence on O, you can argue that the lack of similar abrupt peaks in Ohrid does indicate that Prespa does not have a simple direct influence on the ecology of Ohrid. I would include this in carrying out a general pattern comparison between the two lakes. Ln 24. Four samples is not enough to provide a rigorous comparison of response to a putative Medieval Warm Period; if you did so, it should be in a results section rather than at the end of the discussion. You might mention this earlier if you wish to retain it. The ending on a note of ecological collapse is rather weak. I'd suggest it ends on note of implications for future Quaternary palaeoclimate analysis, since you have not dealt with recent environmental change here.

We would like to thank Dr. Reed for the in depth review of our paper, the comments and suggestions which will help improve the quality of our manuscript. The above mentioned corrections/suggestions will be introduced in the revised version of the paper.

6) You must allude to Lini-Zhang in this paragraph and include the site on the map in Figure 1.

We will add the reference to Zhang et al., (2015). The Lini site (core Co1262) was already marked on the map in Figure 1.

7) p3 ln 11. Needs clearer definition – the inflow from Prespa to Ohrid is logically an external influence. There is no influence of Ohrid on Prespa, so generalizing this as being the internal drivers is rather loose. ln 4. Matzinger only argued that the influence may occur.

As we explained in our reply (comment 1), if we apply the concept of panarchy and consider the lakes as lower scales of the landscape in an interconnected system of lakes, then, the influence of lake Prespa can be considered as internal.

8) The Lini study does not show the pattern suggested on page 2 ln2.

"This decoupling of the response is evident during the MIS 5/4 and 2/1 transitions, when Lake Ohrid displays prolonged and gradual changes." (Cvetkoska et al., 2015). As mentioned before, the Lini sequence extends back to 12.3 ka and does not provide complete insight into the MIS 2/1 transition. At the DEEP site, the diatom record, the TOC and BSi do indicate the gradual change during the MIS 5/4 and 2/1 transitions.

9) ln 27 you need to include the resolution of sample analysis for Prespa, since it is very important to have similar resolution to allow you to consider patterns of change. Even if it was not intentional to achieve this, it is important and should be the x16cm Ohrid and x8 cm Prespa actually do match up. But some comment should be made somewhere about the apparently higher sediment accumulation rate in Ohrid. This is counter-intuitive since an oligotrophic lake has a slower SAR than a meso-eulake. What might be the reason for this?

The sampling resolution for the diatom analyses of the DEEP site and the Co1215 core has been already presented in Table 1. For the SAR, note the different scales cm/a for Lake Ohrid and mm/a for Lake Prespa. In the revised version we will change the SAR to same scale (mm/a) for both lakes and add a comment about the SAR in both lakes.

10) You do not mention that the DEEP results have a surprisingly consistent abundance of FP compared to Co1202. They are not just present in MIS3, so the preliminary interpretation is no longer valid. Why are there more FP???

The sum abundance of facultative planktonic and benthic species and their response is already mentioned in the discussion paper (p. 15059, ln 6−11). However, we will make our statements more clear. The preliminary interpretation of this group in Co1202 in not necessarily invalid, since differences between the results may be also due the different location of the coring sites. Core 1202 was collected from 145m water depth in a coring location to the northeast of the lake and closer to the shore line and more affected by the wind-driven counterclockwise surface currents (Vogel et al., 2010a, b). This may point to spatial differences in the distribution and response of the facultative planktonic and benthic species.

11) In presenting results of PCA you should also include a table of eigenvalues, comparing with Prespa. What proportion of variance is included in Axis 1 and Axis 2. The interpretation of PCA Axis 2 as productivity is not well justified; the multiple ocelli morphotypes plot close to the centre of the diagram and we have no evidence that they indicate higher productivity. In Prespa, we could interpret DCA Axis 2 as productivity on the grounds that there was a clear additional gradient of variation, such that plankton-dominated phases were sometimes dominated by

eutrophic taxa, indicating that P:B ratios were not simply lake level. Here, you need to explore what is driving the full axis 2 variation – there are two long branches and perhaps they are driven by rare occurrence of benthic taxa. If so, you should rerun the PCA on e.g. taxa present at 1% and explain what you are doing. If the proportion of variance in Axis 2 is actually very small, then it should not be viewed as a major gradient of variation.

The eigenvalues and explained variation for the PCA analysis of Lake Ohrid are already presented in Table 1. We thank Referee #1 for this comment, the DCA values for Lake Prespa will be also presented in the revised version. Given the fact that Axis 2 adds ca.15% to the explained cumulative variance of the data we do consider that in this dataset which is dominated by planktonic species it adds additional information. The ordination was already run with all species present at >2% relative abundance. We will add more caution in interpreting the Axis 2 as productivity gradient by comparison to other geochemical proxies to validate our statements.

12) The main point in this section is that glacial phases do not cause the complete loss of diatoms. You compare with Baikal and Hovsgol. These are in Siberia rather than the Mediterranean, so it would be interesting to discuss contrasting climate characteristics here.

The comparison with these records is made to show the uniqueness of Lake Ohrid's diatom record, which is continuous in comparison to other diatom records from ancient lakes.

13) The methods section needs a data analysis section for both sites. Need to justify the use of DCA in Prespa and PCA in Ohrid. What is the gradient length of Ohrid data? Since you are comparing degrees/amplitude of change, you should also state that you have maximised apparent variance by running ordination on species morphotypes at a sub-species level.

The methods used for data analyses in both lakes are compared and referenced in Table 1, while the SD (gradient lengths of the PCA) are presented in Table 2. We will add the results from Lake Prespa in Table 2 and add the suggested statement into the Methods.

14) This really is not true – we are only beginning to understand the complexity of diatom response in Ohrid. You must cite Zhang et al (2015) here, particularly since the counting of very dilute slides has revealed the presence of C. minuscula at the start of the Holocene – could this have been missed in DEEP counting?

We do agree that the diatom response in Lake Ohrid is not just a simple linear relation to the temperature. But, as stated in our reply in the previous section, the long-term trends of changes in species composition in relation to the glacial-interglacial climate, and therefore temperature change are clearly evident at the DEEP site diatom record. On short-time scale, higher complexity underlines the response to different short-term events, like the tephra deposition, the YD and the "8.2" cooling event. We will cite Zhang et al., (2015) regarding their interpretation of the complex responses to the Holocene climate variability at the Lini site, but as apparent from the data, there is probably a spatial variability between the diatom responses at both sites.

C. minuscula is also present at the start of the Holocene at the DEEP site, but also a high percentage of heterovalvate cells of *C. ocellata* which are not observed at the Lini site. Since the aim of this paper is not to go into detailed comparison between the Lini and the DEEP site diatom records, we will focus on the approach to use references from other records to compare our interpretation.

Referee #2

15) Why is the time period up to MIS 5b the target of study? What is the significance of this? p.15052, L 16-18: Meaning unclear. p.15052, L23: Please clarify which terrestrial ecosystems you are referring to in this context. p.15052, L23: Rather than systems evolving sensu stricto, do you really mean that they change in response to climate forcing (patterns repeat)? p.15054, L2 and throughout: Citations should be presented in chronological order. p.15054, L3-4: Which proxies are you referring to? Please state. p.15054, L4-5: Perhaps reword to indicate that your chosen proxies change in response to large-scale climate change. p.15054, L7-8: The proxies to which you are referring need to be stated in each case. p.15054, L22. Need to list these biochemical and geochemical approaches. p. 1505, L1-3. This is one major aim of the research and so should be better integrated. p. 15055, L6: m.a.s.l. Write all acronyms in full on first use. p.15055, L8: 155 and 293m respectively. p.15055, L15: average annual precipitation? p.15055, L 17: from what P concentration? State to give some idea of scale of change. p.15055, L27: if 100% of water input is accounted for, what proportion is groundwater? p.15056, L6: this is rather vague – what aspect of the environment are you referring to? p.15056, L10: please present TP concentrations using the same units of measurement. p.15056, L16: boreholes rather than holes. p.15056, L21: More detail required here. How many cores comprise your composite sequence? p.15056, L20-25: Issue with sentence structure (clarity) p. 15059, L23: More details needed here, re CONISS – was this performed on transformed data? p.15060, L19. p.15060, L21: From the arguments presented it is unclear how 'climate instability' has been inferred. p.15060, L22: You specify that the data are consistent with lower winter temperatures. Again, it is not clear how seasonality has been resolved. p.15060, L24: For clarity, need to highlight that you are referring to Prespa here. p.150061, L19: Circular argument – proxy data can be used to infer, rather than 'correspond to'. p.15061, L24: Presumably there is independent regional evidence for a decrease in precipitation (e.g. speleothems). If not, suggest you be more cautious here and say effective precipitation. p.15062, L7: Phrase 'regime of gradual transition' unclear in this context. p.15062, L9: If there are pollen data available, then please use this as a more robust way of inferring 'glacial aridity and climate cooling'. p.15066, L17: Again, environmental conditions are being invoked (in this case aridity) apparently without basis. Moreover, C. ocellata has hitherto been linked with higher temperatures, yet peaked during your inferred 8.2 ka event, with this diatom assemblage being used to infer cold winters. This does not follow, as it is presented. Please clarify. p.15067, L2: Since there is no apparent compound-specific organic work presented in this paper, you cannot infer definitively that there is a decrease in organic matter preservation – suggest reword to include 'and/or'. p.15067, L2:

'probably associated with lower temperatures'. What is the empirical evidence for this? Any pollen data? Where is your diatom evidence for lower temperatures, since it is claimed that the diatoms in Ohrid are primarily driven by temperature? More detail is needed here if there is a conflict with existing data from Ohrid, and you are trying to persuade the reader that your version of events in more likely. p.15068, L10: It is not clear how seasonality has been resolved in this study. p.15068, L19: Given that you do not find evidence of lake level change in Ohrid during the study period, on what basis do you infer periods of aridity? p.15069, L5: Please describe and explain the feedback mechanisms to which you refer. p.15069, L29: Take care with meaning here. The lake does not have an 'ability' to change as such. In the example given, the lake level increase is what happens to the lake, driven by external forces. It's an artefact of lake morphology that is can fill quickly during wetter periods. Be more cautious about your conclusions here. You acknowledge the uncertainty in the Prespa chronology and thus correlation to Ohrid. The discussion overall is very short. Fig 5 and 6. Given the issues with your assumption that diatom concentrations solely reflect productivity, these figures are probably misleading.

We thank the anonymous referee for the detailed review approach and the provided suggestions and comments. Replies to some of the questions regarding the interpretation of the diatom responses are already given in the first section of our response. We will integrate the suggested changes and clarify the reviewers' queries in the revised version of the manuscript.

16) Figures

Figure 5 will be modified and Figure 6 will be removed from the revised version.

17) SAR data shown but there is no discussion of this anywhere in the text. With such a focus on productivity, sediment accumulation rates should be considered. Diatom concentration data, which is one of the main proxies alluded to throughout, has not been shown in Figs 2 or 4.

The SAR will be discussed along with the DC, DAR and the other 'productivity' related proxies (TOC, BSi). The diatom concentration data was already included in Figure 2 in the discussion paper, but will be also included on Figure 4 in the revised version.

18) No conclusions section is presented.

The lake ecosystem interactions are actually presenting the main conclusions of the comparison between the lakes. In the revised version, we will present short conclusions section to outline the main findings of our study.

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