Title: Complexity of diatom response to Lateglacial and Holocene climate and environmental change in ancient, deep, and oligotrophic Lake Ohrid (Macedonia/Albania)

Associate Editor Tom Wilke

The discussion paper of Zhang et al. has been seen by three referees. All agree that it constitutes a well-done study based on high-quality data and which significantly contributes to our knowledge of diatom response to rapid environmental changes over long time scales. The reviewers also provided very constructive suggestions on how this study can be further improved, particularly in terms of data interpretation. Moreover two referees suggested another round of evaluation ("major revision") and kindly agreed to re-evaluate the revised paper.

We would like to thank the Associate Editor Tom Wilke and the Referees Anson Mackay, Patrick Rioual and Keely Mills very much for their detailed and constructive comments, suggestions and improvements on the manuscript. The main focus of the paper is to explore the complexity of response of diatoms to Lateglacial and Holocene climate and environmental change in Lake Ohrid, which was not apparent in the previous, preliminary, low-resolution diatom studies. Many of the comments focus on a similar theme, in that the complexity may actually be greater than we already acknowledge. The detailed, point-to-point response is given below, and corrections are highlighted in the revised version. Essentially, the main corrections are to add further caution to interpretation of diatom concentration in terms of productivity, and to add caution in interpreting the direct and indirect influences of water temerature change. We now give greater weight to the possible influences of wind forcing and catchment processes in mediating the complex response of diatoms to climate and environmental change with particular respect to temperature.

In their revised version of the paper, the authors should focus on:

- a re-evaluation of their DC interpretations (secondary proxy?),

We explore diatom reponse mainly based on diatom assemblage composition of main planktonic taxa (i.e. ecology) rather than diatom concentration. We welcome the opportunity for deeper probing of the value of diatom concentration data in palaeolimnological research. We now add caution to interpretation of diatom concentration in terms of productivity, and assess qualitatively the influences of sedimentation rate and diatom valve size on concentration data. As discussed in the revised version, diatom concentration could still indicate a real change in lake productivity during the early Holocene, and relatively high diatom concentration during the mid Holocene may be largely attributed to low sedimentation rate.

- a comparison with another geochemical proxies (e.g., TN and δ18O), and

We incorporate comparison with calcite $\delta^{18}O$ data from core Co1262 and other Lake Ohrid sediment cores (Leng et al., 2010; Lacey et al., 2015). The core Co1262 TN data (not shown) from Lacey et al. (2015) display very similar trends to TOC in indicating organic matter content, so it is not necessary to show both and here we just show TOC which is usually used.

- a more comprehensive discussion of anthropogenic impacts on diatom communities (possibly involving TN and/or δ15N proxies).

We have no $\delta^{15}N$ data in this study. TN is closely correlated with TOC in indicating organic matter content, which is derived mainly from in-lake algal production (Lacey et al., 2015). We discuss anthropogenic impact on diatom composition during the late Holocene through comparison with pollen-inferred anthropogenic catchment deforestation and also based on the preferences of diatom species on nutrient enrichment such as external P and N input associated with deforestation.

I also encourage the authors to compare the Co1262 diatom record with the records recently obtained from site DEEP and to link their study more closely to the ongoing pyroclastic work in Lake Ohrid (see, for example, the diatom response and geochemical studies in the current special issue).

Thank you. We now incorporate comparison with low-resolution diatom data recently obtained from the DEEP site (Cvetkoska et al., 2015) and also plot the location of the DEEP site in the lake in Fig. 1. The main focus of this paper is to examine diatom response in greater detail than previously. To strengthen diatom interpretation of core Co1262, we compare with geochemical data primarily from the same core and where appropriate, with those from other sediment cores. Detailed comparison with all geochemical data from existing sediment cores is not necessary and is also beyond the focus of the paper. This is partly because recent geochemical studies on the DEEP site in this special issue focus mainly on the glacial–interglacial timescale rather than the Holocene, so cannot provide the same detail as the higher-resolution analysis of this study.

Judging from the response of the authors, I feel that these questions can be adequately addressed. I therefore invite the authors to submit a revised version for re-consideration, which is based on their responses to the suggestions of the referees published during the interactive discussion.

Thank you. We have modified accordingly.

Referee Anson Mackay

General comments:

This is a high resolution, palaeoecological study of a late glacial/Holocene sequence from Lake Ohrid, the oldest lake in Europe. The main focus of the study is on diatoms—silica-walled single-celled algae, which because they are at the base of aquatic foodwebs are important for ecosystem functioning of lakes. Given the importance of biodiversity and endemism in Lake Ohrid, understanding diatom variability over long-time scales and during periods of rapid environmental change is of considerable importance.

Overall, this is a well-writen study that details methods and results clearly and succinctly. The quality of tha data is excellent. I do have thoughts on the interpretation of the data, which I invite the authors to consider.

Thank you.

(i) The authors assume that high diatom concentrations = high productivity. However, diatom concentrations can be a rather poor indicator of productivity, and concentrations are often more related to cell size and biovolume (e.g. see Battarbee et al. 2001).

Thank you. We have incorporated at the end of the section “Material and methods” that diatom concentration data can be influenced both by differences in cell sizes of diatom species and by changes in sedimentation rates. Due to the lack of diatom cell biovolume data and dry sediment bulk density data in this study for calculating diatom biovolume accumulation rates, we assess qualitatively in the section “Interpretation” the potential influences of sedimentation rates and valve sizes of main planktonic taxa (including size classes of *Cyclotella fottii* complex which has a wide range in diameter), and are careful not to over-interpret fluctuations in concentration.

(ii) The authors then assume that high diatom “productivity” (based on concentrations = high temperatures. But I’m not sure that “productivity” in itself should used to infer temperature, as productivity will also be influenced by nutrient inputs from the catchment, nutrient regeneration (through winds and turnover etc). Also, what does temperature mean in this context—water or air temperature? Summer or mean annual temperature etc? These are not clear.

We do not simply assume that productivity varies directly with water temperature; instead, we discuss the high complexity of diatom response to Lateglacial and Holocene water temperature change and have now given greater weight to disscusion of the possible influences of wind forcing and catchment mediation—thank you. Our discussion is based mainly on diatom assemblage composition of the dominant planktonic taxa (i.e. ecology) rather than diatom concentration, in acknowledgement of potential weakness of diatom concentration as a productivity proxy. We now justify more clearly in the text how we assess the influences of multiple parameters. For example, to assess the possible influence of catchment dynamics and nutrient delivery, we compare the diatom results of core Co1262 with sedimentological potassium (K) intensity and sedimentation rate data from the same core (Wagner et al., 2012; Lacey et al., 2015), with palynological data from previous lake sediment cores in Lakes Ohrid and Prespa (Wagner et al., 2009; Panagiotopoulos et al., 2013), and with calcite $\delta^{18}O$ data from existing sediment cores in Lake Ohrid (Leng et al., 2010; Lacey et al., 2015). In-lake mixing-induced nutrient regeneration could be driven by epilimnetic water cooling and wind forcing, as complete lake circulation usually occurs in severe winters or following intense wind action in less severe winters in this lake today (Stanković, 1960). Apart from discussing the direct response of diatoms to water temperature-induced lake productivity, proposed by Reed et al. (2010) on a glacial/stadial-interglacial/interstadial scale, we interpret water temperature-related stratification/mixing and epilimnetic nutrient availability as an indirect response of diatoms to water temperature change. Average wind speed in winter is higher than in summer today (Stanković, 1960), and to aid discussion of the possible influence of wind forcing, we have compared particularly with the globally-stacked proxy mean annual surface temperature record (Shakun et al., 2012; Marcott et al., 2013). The high complexity of limnological response compared to the over-simplistic conclusions drawn in the longer timescale but lower-resolution analysis of Reed et al. (2010) is indeed the most important finding of this study. We have clarified that "temperature" here is "mean annual epilimnetic water temperature", in abbreviation "water temperature". Although temperature is homogeneous in the entire water column during the period of complete lake circulation, this originates from the decrease in epilimnetic water temperature. Although lake circulation and possible lake ice cover (during the Lateglacial period) occur in winter and lake stratification occurs in summer, we assume that this study reflects mean annual temperature due to the long-timescale (multi-centennial to millennial) analysis with the seasonality indivisible in the diatom data.

(iii) As diatom concentrations were so low prior to 10 kyr BP, is the dominance of *C. fottii* as portrayed by relative abundance in Fig 5, real? It would be interesting here to convert the principal species highlighted in Fig 5 to concentration data. It may be that numbers of *C. fottii* are actually very low and therefore one could draw different conclusions about the limnology of Ohrid during the early Holocene from high numbers of *C. fottii* during the mid Holocene.

Thank you for the suggestion of taking into account the concentration data for the principal species, particularly *C. fottii*. We actually did this in an earlier version of the manuscript, but, for example, it proved difficult then to compare the concentration of *C. fottii* during the Lateglacial and earliest Holocene with that during the mid Holocene, as sedimentation rates during the two periods differ largely and concentration data are of course influenced by the changing sedimentation rates. As a supplement to the relative abundance data of diatom species, it would be more useful to compare the influx data (with the influence of sedimentation rate factored out) if we had dry bulk density data, but unfortunately these data are not available. In this lake, "calcite content" has proved to be a strong proxy for temperature-induced lake productivity (Vogel et al., 2010; Wagner et al., 2010). In combination with calcite content and other geochemical data, we discuss complex responses of diatom composition to low water temperature during the Lateglacial and earliest Holocene and to high water temperature during the mid Holocene, respectively. High *C. fottii* relative abundance, without the occurrence of *C. ocellata*, during the Lateglacial (and earliest Holocene) responds to low lake productivity in relation to low water temperature, which, together with low diatom concentration (i.e. low concentration of *C. fottii*), is consistent with MIS 2 diatom results from previous analyses (e.g. Wagner et al., 2009; Reed et al., 2010). High *C. fottii* relative abundance and more surprisingly low *C. ocellata* abundance during the mid Holocene are probably a response to nutrient limitation in the epilimnion as a result of high temperature-related stratification (i.e. low internal nutrient upward supply), low catchment erosion (i.e. low external nutrient input) and the calcite-scavenging effect (i.e. high phosphorus precipitation). However, relatively high diatom concentration (and high concentration of *C. fottii*, not shown) is largely related to low sedimentation rate during the mid Holocene.

I think therefore that aspects of the Discussion and Conclusions will need to be changed.

We have modified accordingly.

My specific comments are detailed below.

Specific comments:

Introduction:

I recommend adding an extra paragraph detailing important features of the climate variability during this period in the eastern Mediterranean, leading to what the aims or hypotheses are being addressed in this study.

Thank you. We have added that temperature reconstructions during the Lateglacial and Holocene in the northeastern Mediterranean region are rare and rely mainly on pollen data (e.g. Bordon et al., 2009; Dormoy et al., 2009; Pross et al., 2009). Using pollen as a temperature proxy in this region is controversial. Renssen et al. (2012) suggested that pollen-based temperature reconstruction may be unreliable since precipitation rather than temperature is the main climatic control on Mediterranean vegetation distribution, while Mauri et al. (2015) argued instead that pollen transfer functions can provide robust results for temperature reconstruction in this region. Quantitative climate reconstruction methods have their own stengths and weaknesses (Birks et al., 2010; Juggins and Birks, 2012), and pollen-based temperature reconstructions can show different patterns and amplitudes of change depending on the technique used (Dormoy et al., 2009; Peyron et al., 2013). Deep Lake Ohrid, wherein no major lake-level change during the Lateglacial and Holocene has been reported (Wagner et al., 2009; Reed et al., 2010), is arguably an ideal site for using palaeolimnological proxies such as diatoms to improve understanding of temperature change in this region.

I myself would be cautious of ascribing diatom changes to temperature. It is the case that diatom productivity during glacials in many lakes is usually lower than during e.g. interglacials. Temperature is likely to be one factor linked to lower productivity, but others will include extended snow and ice cover (if relevant), potentially fewer nutrients entering the lake from catchments etc.

We include in the section “Interpretation” discussion of the potential influences of ice cover and nutrient delivery as suggested. For example, during the Lateglacial, ice cover is clearly indicated by the deposition of ice-rafted debris inferred from the occurrence of gravel grains at the coring site of the lake (Wagner et al., 2012), and facultative planktonic fragilaroid taxa are probably related to cold water and winter lake ice cover (Mackay et al., 2003; Schmidt et al., 2004); nutrient delivery from the catchment was high during this period, as suggested by high sedimentation rate, high K intensity and low authigenic matter content and also supported by sparse vegetation and unsettled soils in the catchment (Panagiotopoulos et al., 2013); nutrient regeneration from the bottom layer was also high, under low surface water temperature and intense wind action, even probably dimictic or monomictic rather than currently oligomictic. Thus, the dominance of *C. fottii* without the occurrence of epilimnetic species during the Lateglacial is linked to low temperature rather than nutrient limitation.

However, it is great to see the findings from this study be compared to previous work on the lake.

Thank you.

Site Description:

This is excellent.

Thank you.

Materials and methods:

Very good account of chronological framework, based on previously published studies, including authors of the current study.

Thank you.

Very useful account of diatom taxonomies and their brief history in the context of Ohrid diatom research.

Thank you.

Results:

It would be useful to start off the results with a statement of how many diatom samples were analysed, and their approximate resolution. Also, how many species were identified in total. Otherwise these are fine.

Thank you. We introduce the number (104 samples in total) and resolution of diatom samples (including both sample interval and age resolution) in the section "Material and methods". We have now added at the start of the section "Results" that a total of 99 diatom species was identified, consisting of nine planktonic species, five facultative planktonic species and 85 benthic species. In spite of low diversity of plankton, its relative abundance is >90% throughout the record.

Interpretations:

P11, Line11: during the Holocene, temperature changes are only very modest, and so unlikely to drive diatom productivity alone. Therefore, I would omit end of this sentence "…both in relation to temperature change" as changes in the mixing regime could also be brought about by changes in wind / storminess.

We have deleted this sentence here, and reworded it to "in contrast to previous, preliminary, and low-resolution diatom studies (Roelofs and Kilham, 1983; Wagner et al., 2009; Reed et al., 2010; Cvetkoska et al., 2012), variations in the relative abundance of these taxa may be a direct response to shifts in temperature-induced lake productivity, and we should also consider the possible influences of temperature-related stratification/mixing regime, wind forcing, catchment mediation, light limitation, and/or spring inflow" in the following paragraph in the revised version.

P11, line 12-14: it is probalby the case that dissolution is minor but as only one species was considered, the index used here might be of limited value for other, potentially more susceptible taxa.

Cyclotella fottii is an endemic taxon in this ancient lake and is also abundant throughout the record. It consists of various morphotypes in cell size, ranging from <10 µm to >50 µm (Fig. 3). It is arguably an ideal taxon to be taken to evaluate the dissolution, and the index shows that dissolution is minor.

We showed in Reed et al. (2010) that inclusion of the smaller taxon *Cyclotella ocellata*, which is more fragile, gave low values for phases which were intuitively better preserved.

Fig 3: it's great to see so much care taken into characterising the different size classes for *Cyclotella fottii*, and for the ocelli number for *Cyclotella ocellata*. This makes Fig 3 look very busy therefore, and if these criteria are not showing anything ecologically meaningful, I think it would be better to just combine and present as one taxa for each. Although I do see that this has indeed been done in Fig 4.

Thank you. Although up-to-date information does not show any clear ecological difference for *C. fottii* size classes and *C. ocellata* morphotypes, respectively, diatom counting in this study follows previous studies in this lake (cf. Reed et al., 2010; Cvetkoska et al., 2012) to investigate possible sub-species response, and meanwhile size classes of *C. fottii* could help to assess qualitatively the potential influence of diatom cell sizes on concentration data. Thus, we would keep these as Fig. 3.

Note that diatom concentrations by themselves are not terribly good predictors of diatom productivity because as diatom cells get smaller, their concentration often increases (Battarbee et al. 2001); Rioual and Mackay 2005), e.g. in Ohrid, concentrations look to be drvien by changing abundances of the small-celled *C. ocellata*.

Thank you. As mentioned above, we have incorporated discussion of the possible influence of diatom cell sizes on concentration data. Since sedimentation rate in Zones D-2 and D-3 is nearly unchanged compared to Zone D-1 (Fig. 4), high diatom concentration in Subzone D-2a and Zone D-3 might be attributed to declined abundance of large *C. fottii* morphotypes and increased abundance of relatively small *C. ocellata*, but high abundance of very small *C. minuscula* in Subzone D-2b is correlated with low rather than high diatom concentration (Fig. 3). Thus, diatom concentration could still indicate a real change in lake productivity during the early Holocene. Also, during the mid Holocene, relatively high diatom concentration is correlated with low rather than high abundance of smaller valves of *C. ocellata* and *C. minuscula*, and this is probably due to low sedimentation rate.

For the F-index, why not change the x-scale, so that more variation in the data can be seen (e.g. 0.6-1.0).

We plot the F index values, which are >0.75 throughout the record, to show that diatom preservation quality is high, and not to track the variations.

In Fig 4, diatom concentrations show v similar trend to TOC during early Holocene, but to c. 8 kyr BP, but look to have different trends between 8-3 kyr BP. Throughout most of the sequence, TOC looks to be negatively correlated with K (clastic input), so TOC is being governed to a certain extent by catchment in-wash?

Lacey et al. (2015) discussed sources of organic matter in this lake through analysing the same sequence as in this study, and inferred that it is dominantly of algal origin based on the TOC/TN, HI and OI data (the latter two datasets plotted in Fig. 4 in this manuscript). TOC/TN ratios are low (<12) indicating a mainly algal source. Rock Eval data plotted on a van Krevelen-type diagram (HI vs OI) also suggest predominantly algal-derived (Type II) organic matter (Lacey et al., 2015). As mentioned above, diatom concentration could indicate a real change in lake productivity during the early Holocene, while it is largely affected by low sedimentation rate during the mid Holocene and diatom growth is also restrained by nutrient limitation in the epilimnion. This is probably why diatom concentration shows a similar trend to TOC during the early Holocene but a different trend during the mid Holocene. It is not unusual that TOC and K are anti-correlating, and this is not necessarily due to catchment influence. Whereas TOC represents the organic matter content in the sediment, K represents the clastic content. Organic matter and clasts could dilute each other. If there is high productivity in the lake and high accumulation of organic matter in the sediment, K is low, probably just due to dilution by organic matter (and vice versa). Thus we have evaluated catchment erosion and nutrient delivery through considering sedimentation rate, K intensity (i.e. clastic content) and calcite and organic matter content (i.e. authigenic matter content) together. For example, during the mid Holocene, low sedimentation rate, low K intensity, and high calcite and organic matter content suggest low catchment erosion, which is supported by by dense vegetation and stable soils in the catchment (Wagner et al., 2009) and by a drying trend and reducing water inflow from rising calcite $\delta^{18}O$ values (Leng et al., 2010; Lacey et al., 2015). In other words, we can only interpret the TOC and K data through comparing with other proxies. Still taking the mid Holocene as the example, high TOC content could indicate high algal organic matter production, which is supported by high calcite content, high HI and low OI (Lacey et al., 2015); low K concentration could indicate low clastic delivery (rather than due to dillution effects), which is supported by low sedimentation rate, densely-vegetated catchment (Wagner et al., 2009) and reducing water inflow (Leng et al., 2010; Lacey et al., 2015).

In Fig 5, it's pleasing to see that there really is very good correspondence between diatoms from different sites.

Thank you.

Palaeoecology interpretations:

P12, Line 18: Reference to ice-rafted debris is made quite a lot (Wagner references) –would be good to provide more detail. Is there is a proxy for IRD in Ohrid that could be plotted on Figs 4-5.

The "silt with dropstones" at the lowermost section in "lithostratigraphy" in Fig. 3 corresponds to the ice-rafted debris deposition, with the occurrence of coarse sand and gravel grains interpreted as IRD (e.g. Vogel et al., 2010; Wagner et al., 2012). We have clarified this in the text. We have realised that sometimes, there are some misunderstandings with the term "IRD" as it does not refer to "transportation by glaciers" but to transportation with ice floes from the lake ice cover.

A couple of thoughts on K as an index of clastic meterial. I assume that this is being brought into the lake via fluvial input, and may therefore be an indicator of wetter conditions, or less stable soils in the catchment. But I note that in Wagner et al. 2012, K is also linked to tephra material –so this indicator actually has complex sources which will need to be highlighted sooner (although you do state this on P15, Line 12).

As mentioned above, we have considered sedimentation rate, K intensity (i.e. clastic content) and calcite and organic matter content (i.e. authigenic matter content) together to assess catchment erosion and nutrient delivery. We also compare with palynological data for catchment vegetation change from previous lake sediment cores in Lakes Ohrid and Prespa (Wagner et al., 2009; Panagiotopoulos et al., 2013), and with calcite $\delta^{18}O$ data from existing sediment cores in Lake Ohrid (Leng et al., 2010; Lacey et al., 2015). When there is a tephra, K intensity can be high depending on the chemical composition of the tephra. The correlation of peak K intensity with the Mercato tephra is supported by strontium (Sr) intensity, both elements originating from volcanic glass shards (Wagner et al., 2012). The presence of volcanic glass shards was verified by microscopic and geochemical fingerprint analyses (Wagner et al., 2012). This also implies that the tephra impact on K intensity is restricted to distinct horzions corresponding to the tephra layer, as volcanic ash is commonly deposited in a few weeks, and thus this does not affect the overlying long-term trend in the data.

It's interesting to note that K and TOC show quite negative responses, that is as K increase, TOC increases and vice versa. Does this mean that they are tied to a similar source? Do the authors have any indication from CN ratios (for example) that point to the origin of the TOC in the lake sediments?

Please see the reply above.

P13, Line 24: I don't think that it's correct to infer diatom productivity from concentrations alone – see comments above. Therefore link between temperature and productivity likely too simplistic. (Also see P15, lines1-4)

Please see the reply above about diatom concentration. In contrast to previous, preliminary, and low-resolution diatom studies (cf. Wagner et al., 2009; Reed et al., 2010), this study demonstrates a complex diatom response to climate and environmental change with particular respect to water temperature, probably comprising a direct response to temperature-induced lake productivity in some phases and an indirect response to temperature-related lake stratification/mixing and epilimnetic nutrient availability in others. We have also demonstrated the possible influences of physical limnological (e.g. the influence of wind stress on stratification/mixing) and chemical processes (e.g. the influence of catchment dynamics on nutrient input) in mediating the complex response of diatoms.

P15, bottom off: interesting that diatom flora shows no response to potential 8.2 event

Yes, although we improved the analytical resolution of diatoms around this putative event.

P16, Line 6: again, stating that the mid Holocene was one of high temperature – you should really refer to a global consensus record (such as Marcott et al. 2013) for such a claim.

Thank you. We have incorporated comparison with the globally-stacked proxy mean annual surface temperature record (Shakun et al., 2012; Marcott et al., 2013) in the section "Interpretation".

Conclusions:

The conclusions are written in context of temperature forcing changes, but no temperature records are actually provided.

Thank you. We have added that our multi-proxy analysis reveals that water temperature was low during the Lateglacial and even the earliest Holocene, was high during the early Holocene and reached the maximum during the mid Holocene.

Referee Patrick Rioual

General comments:

In this manuscript Zhang et al. aim at reconstructing Lateglacial and Holocene climate and environmental changes by using the diatom sedimentary record of Lake Ohrid. As mentioned by the authors, the ancient Lake Ohrid is a key site for palaeoclimate research in the northern Mediterranean region. The diatom sequence presented in this manuscript includes 104 samples. The diatom analysis of the samples is of high quality, making good use of previous taxonomic works on the endemic species of Lake Ohrid. In addition to calculating diatom relative percentages and concentration in the sediment, the preservation of diatom valves was also assessed. This work represents an important diatom dataset that warrants publication. The manuscript is well written and the figures are of good quality.

Thank you.

However, I'm concerned with the interpretation of the sequence, especially with the assumption (page 14353, lines 9-11) that the observed diatom shifts relate to changes in productivity that are themselves associated to change in temperature.

We have deleted this sentence here, and reworded it to "in contrast to previous, preliminary, and low-resolution diatom studies (Roelofs and Kilham, 1983; Wagner et al., 2009; Reed et al., 2010; Cvetkoska et al., 2012), variations in the relative abundance of these taxa may be a direct response to shifts in temperature-induced lake productivity, and we should also consider the possible influences of temperature-related stratification/mixing regime, wind forcing, catchment mediation, light limitation, and/or spring inflow" in the following paragraph in the revised version.

1. The relationship between diatom concentration and palaeoproductivity As acknowledged by the authors (page 14351, lines 1-4), diatom concentration data alone are not ideal for estimating palaeoproductivity. The lack of dry bulk density data prevented the authors from calculating diatom influx. However, in this dataset like in the study by Rioual and Mackay (2005) that the authors mentioned, there are very large differences in biovolume between the dominant planktonic diatoms. These differences in biovolume are actually very important when considering productivity, at least as much as the changes in diatom concentration and certainly more important than the changes in sediment accumulation rate and dry bulk density. For example, a medium-sized frustules of C. fottii with a diameter of 25 micrometers has a biovolume ~240 times larger than the average frustules of C. minuscula (diameter 3-5 microns) and ~50 times larger than an medium-sized C. ocellata (with a diameter of ~10 microns). If one considers the size of the main diatoms, the estimates for palaeoproductivity will change a lot. For example during the Younger-Dryas (zone 1a), the diatom concentration was very low at ~2 x 107 valves.g-1 but the assemblages were dominated by large-celled C. fottii. If one estimate the BVAR (by taking the bulk density as constant), one will find that the planktonic diatom "productivity" was almost as high as during the early Holocene (zone 3a) when diatom concentration peaked (80 x 107 valves.g-1) but when assemblages were dominated by the smaller diatom C. ocellata. Obviously, this changes the interpretation of the sequence. The difference in cell-size is also very important to understand the ecology of Cyclotella species (see the recent paper by Jewson et al. 2015 for details). In my opinion, the authors should estimate the Biovolume Accumulation Rate (BVAR), to improve their estimate of palaeoproductivity as the curve of diatom concentration is a rather poor proxy record for this sequence from Lake Ohrid. The authors have already split into size-class the valves of C. fottii when counting, so the biggest part of the work has been done. As I mentioned earlier, they could use a constant for dry bulk density as doing this will not change the results significantly.

Thank you very much for demonstrating the significance of diatom cell size in using diatom concentration to estimate lake productivity. Although *C. fottii* has already been split into size classes, we find that it is still impossible to estimate the mean valve diameter and cell biovolume of main planktonic diatom taxa using our available data. Thus we cannot estimate the diatom biovolume accumulation rate, although it is a good idea to set dry bulk density roughly as a constant. However, we have assessed qualitatively the potential influence of valve sizes of main planktonic taxa on diatom concentration. During the early Holocene (Zones D-2 and D-3), since sedimentation rate is nearly unchanged compared to Zone D-1 (Fig. 4), high diatom concentration in Subzone D-2a and Zone D-3 might be attributed to declined abundance of large *C. fottii* morphotypes and increased abundance of relatively small *C. ocellata*, but high abundance of very small *C. minuscula* in Subzone D-2b is correlated with low rather than high diatom concentration (Fig. 3). Thus diatom concentration could still indicate a real change in lake productivity during the early Holocene. During the mid Holocene, relatively high diatom concentration is correlated with low rather than high abundance of smaller valves of *C. ocellata* and *C. minuscula*, and this may be largely attributed to consistently low sedimentation rate.

2. The relationship between palaeoproductivity and temperature The second part of the assumption that relates productivity with temperature is also problematic. This is because populations of most planktonic diatoms in temperate lakes can generally grow in a wide range of water temperature and primarily respond to other factors such as light and nutrients (e.g. Anderson 2000). In any case, the temperature range in which these diatoms can grow is much larger than the variations in temperature known to have occurred during the Holocene. It is therefore very unlikely that temperature was the main factor driving the changes observed in this diatom sequence. Most likely, the availability of nutrients (and light?) was more important as driving factors behind the observed changes in productivity. As mentioned several times in the manuscript, it is likely that changes in mixing regime and their effects on the availability of nutrients were decisive factors explaining the shifts in diatom species. It is also important to remember that lake circulation is not only driven by temperature but also by wind, and seasonal changes in solar radiation. Therefore, I think that you should focus more on change in stratification/mixing regime rather than on temperature as a basis for interpreting this diatom sequence. In addition to the mixing regime, and considering the ecology of C. fottii, one would expect changes in the light regime and/or the depth of the photic zone (see Saros & Anderson 2015) to have a very large influence on the abundance of this hypolimnetic species. It is also important to consider that Lake Ohrid is a karstic system and as mentioned in the manuscript (Site description, p. 14347) it is mainly fed by karstic springs, including Lake Prespa underground outflow. According to Lorenschat et al. (2014) this hydrological setting has a strong influence on the nutrient budget of Lake Ohrid, as water from Lake Prespa is nutrient-rich compared to that of Lake Ohrid. Therefore an increase or decrease in the outflow from Lake Prespa may influence the nutrient budget of Lake Ohrid and its diatom assemblage. Another point that the authors should consider is the possible effects of biotic interaction on the diatom assemblages. Apparent decrease in diatom productivity may be due to

competition from the other primary producers in the lake. In the littoral zone, chlorophyceae including Cladophora spp and Chara spp. are the dominant primary producers. These algae can act as a sink for nutrients for nitrogen and phosphorus.

Thank you. Wagner et al. (2009), Reed et al. (2010) and Cvetkoska et al. (2012) have suggested a simple and strong response of diatoms in Lake Ohrid to temperature-induced changes in lake productivity during the last glacial–interglacial cycle. This study focuses on the Lateglacial and Holocene period to test diatom response to water temperature change in greater detail, and reveals that diatom response during this period is complex. Diatoms could respond to water temperature change in a direct way through responding to temperature-induced lake productivity, for example between ca. 9,500–8,200 cal yr BP, or in an indirect way through responding to temperature-related lake stratification/mixing and epilimnetic nutrient availability, such as during the mid Holocene. We have now given greater weight to the possible influences of wind forcing and catchment process in mediating the complex response of diatoms. To assess the possible influence of catchment processes on nutrient delivery, we compare the diatom results of core Co1262 with sedimentological K intensity and sedimentation rate data from the same core (Wagner et al., 2012; Lacey et al., 2015), with palynological data from previous lake sediment cores in Lakes Ohrid and Prespa (Wagner et al., 2009; Panagiotopoulos et al., 2013), and with calcite $\delta^{18}O$ data from existing sediment cores in Lake Ohrid (Leng et al., 2010; Lacey et al., 2015). Average wind speed in winter is higher than in summer today (Stanković, 1960), and to aid discussion of the possible influence of wind forcing, we have compared particularly with the globally-stacked proxy mean annual surface temperature record (Shakun et al., 2012; Marcott et al., 2013). As for the possible influence of light availability, Lake Ohrid is still highly oligotrophic and exceptionally transparent (Matzinger et al., 2006a, 2007) and hypolimnetic diatoms can be found at >200 m water depth (Stanković, 1960), so light limitation can be assumed insignificant. As for spring inflow and associated nutrient transport from Lake Prespa, the short core in Lorenschat et al. (2014) was recovered close to the Sveti Naum springs in the southeastern part of Lake Ohrid, while it is apparent that spring inflow does not reach the site of core Co1262 in the westernmost part of the lake (Matzinger et al., 2006a), and thus the direct influence of springs is probably negligible. As for biotic interaction, we agree with you that it is a potential factor influencing diatom composition, and we have incorporated this into interpretion of the disparity between high algal production and low abundance of epilimnetic diatoms during the mid Holocene.

Specific comments:

Abstract Page 14345, line 4: I don’t think you can use the term “high-resolution” for this study as the temporal resolution of the sampling is at best multi-decadal, and most often centennial.

Thank you. We have deleted this term.

Results Page 14351, lines 21-22: change to: “with a peak in diatom concentration. . .”

We have modified this.

Interpretation Page 14353, line 14: re-write such as: “. . . but represent real ecological shifts.”

We have modified this.

Page 14353, lines 15-17. Here you should add a sentence to clearly state your interpretation for the curve of PCA axis-1 scores, something like: “Axis-1, therefore, appears to reflect a gradient of mixing regime, and opposes assemblages dominated by epilimnetic taxa associated with strong thermal stratification to that dominated by C. fottii, associated with the hypolimnion and stronger lake circulation.

We do not interpret diatom PCA Axis 1 scores, because diatom response is complex, non-linear and not straightforward.

Page 14353, line 16: change such as: "with high positive scores associated with the dominance. . ."

We have modified this.

5.1 The Lateglacial: Page 14354, Lines 3-4 you wrote: ". . .C. fotti indicates low temperature-dependent lake productivity. . .". I think this statement may not be correct. (page 14351, lines 1-4). As discussed above, the large biovolume of C.fottii largely "compensate" for the low diatom concentration. Planktonic diatom productivity was therefore already high during the YD, at least in the same order of magnitude as during the Holocene.

As mentioned above, our discussion is based mainly on diatom assemblage composition of the dominant planktonic taxa (i.e. ecology) rather than diatom concentration. According to the up-to-date information of the ecology of *C. fottii*, we interpret the low-diversity dominance of *C. fottii* during the Lateglacial as low lake productivity in relation to low water temperature, and this is consistent with other proxy data from the same core (Lacey et al., 2015) and with previous low-resolution diatom data from other sediment cores in this lake (Roelofs and Kilham, 1983; Reed et al., 2010). Although the biovolume of *C. fottii* is generally high, low productivity during the Lateglacial could be reflected by low diatom concentration, and this is consistent with previous diatom studies on the last glacial–interglacial cycle (Roelofs and Kilham, 1983; Wagner et al., 2009).

Lines 13-15: Long duration of complete lake circulation: In my opinion, this is the most important point for the interpretation of the diatom assemblage in this interval because a large diatom like C. fottii requires turbulence to remain in suspension in the water column.

We agree with you. We have incorporated in the section "The mid Holocene" that, although *C. fottii* is similarly at high abundance in Zones D-1 (the Lateglacial) and D-4 (the mid Holocene), the abundance of large *C. fottii* forms in Zone D-4 is much lower (Fig. 3) and fewer cells can complete their full life cycle with sexual reproduction (Stoermer et al., 1989) possibly linked to strong stratification during the mid Holocene.

Lines 21-22 you wrote: "erosion-induced external nutrient input would also have been high". This statement may not be correct. The geochemical evidence indicates more clastic delivery, but clastic material does not necessarily provide nutrients (nitrogen, phosphorus) for diatoms, except maybe silica.

We have strengthened that, high sedimentation rate, high K intensity (i.e. high clastic content), and low calcite and organic matter content (i.e. low authigenic matter content) suggest high catchment erosion, and this is supported by sparse vegetation and unsettled soils in the catchment during the Younger Dryas. Thus, erosion-induced external nutrient input would have been high.

Line 23: again, productivity may not have been that low if you consider the biovolumes.

We agree with you, and if considering the *C. fottii* biovolume, lake productivity would have been less lower, but still lower than that during the Holocene.

On lines 23-27, you mentioned temperature reconstructions for the Younger-Dryas for the study region. Here it would help the reader if you could give some estimated values by how much temperature decreased in that region during the YD.

Due to the qualitative analysis of diatoms and other proxies in this study, we cannot provide an estimated value of the YD water temperature decrease. Although there are pollen-based quantitative air temperature reconstructions in the southern Balkan region, we have pointed out in the section "Introduction" that pollen-based temperature reconstructions can show different patterns and amplitudes of change depending on the technique used.

5.2 The earliest Holocene (page 14355) Line 5: add "in the epilimnion" after "reduced nutrient availability"

We have modified this.

Lines 7-9: In my opinion, the decrease in small, facultative planktonic, Fragilariaceae essentially suggests a decrease in horizontal transport from the littoral zone (where these diatoms essentially live) to the pelagic zone (from where the sediment core was retrieved). So this is associated with shorter, less intense period of lake circulation. The way it's written, it almost suggests that these Fragilariaceae are "ice-diatoms" associated to ice-rafted debris.

Thank you. We have incorporated this into interpretation.

Line 13: increase in algal organic matter contribution: would it be possible than algae, other than diatoms caused this increase? For example chlorophyceae are also present in the pelagic zone (Miho & Lange-Bertalot 2003) and in the littoral zone (Schneider et al. 2014).

Here the subtle increase in algal organic matter production corresponds to the slight increase in the abundance of *C. minuscula*, and thus there is no evidence to attribute this to other algal groups.

Lines 14-15: again, the average size of C. fottii appears to increase in that interval as shown in Fig.3 with a decrease in the percentages of cells <20 microns and an increase in the percentages of cells of large size classes (> 20 microns). So the productivity of pelagic diatoms probably increased in comparison with the previous interval.

The increase in the abundance of large *C. fottii* morphotypes (>20 μm) may be compensated by the increase in small *C. minuscula* (Fig. 3).

5.3 The early Holocene (page 14356) Lines 6-7: Here the large increase in diatom concentration is mainly caused by small (C.ocellata) and very small (C. minuscula) diatoms. So overall, the diatom pelagic productivity may have in fact decreased by comparison with the previous interval.

As mentioned above, high diatom concentration in Subzone D-2a and Zone D-3 might be attributed to declined abundance of large *C. fottii* morphotypes and increased abundance of relatively small *C. ocellata*, but high abundance of very small *C. minuscula* in Subzone D-2b is correlated with low rather than high diatom concentration (Fig. 3). Thus, diatom concentration could still indicate a real change in lake productivity during the early Holocene.

Lines 7-12: Interestingly, C.ocellata has been reported to be favored by high nitrogen concentrations including from Lake Prespa, the sister lake of Lake Ohrid (Kocev et al. 2010).

Thank you. We have incorporated this in the introductory part of the section "Interpretation".

Lines 19-21: Again this statement is most likely wrong, as productivity probably decreased not increased if you take into account biovolume instead of concentration.

As discussed above, diatom concentration could still indicate a real change in lake productivity during the early Holocene, and we explore diatom reponse based mainly on diatom assemblage composition of the dominant planktonic taxa (i.e. ecology) rather than diatom concentration.

Lines 23-25: In Lake Ohrid, calcium-rich waters that originate from subaquatic springs significantly enhance lacustrine carbonate precipitation (Matter et al. 2010). Is it possible that the observed peak in calcite was caused by increase input of spring water?

We agree with you that increased inflow of spring water may increase the Ca^{2+} and HCO_3^- concentration and then promote calcite precipitation. However, calcite precipitation in this lake is primarily attributed to photosynthesis and the contribution of calcite precipitated around spring areas is minor (e.g. Vogel et al., 2010; Wagner et al., 2010). Also, Matter et al. (2010) are referring to coastal areas, and the coring site of this study is away from the direct influence of springs.

Page 14357, lines 2-6: The absorption of phosphorus on calcite particles is a very interesting explanation.

Thank you.

Page 14357, lines 19-2: Most Stephanodiscus species prefer by low Si/P conditions, which also suggests that the potential influx of silica associated with the tephra layer had no lasting influence. Is it possible that this species of Stephanodiscus was stimulated by an increase in the strength and/or length of the mixing period? Increase mixing would also promote the percentages of C. fottii.

Thank you for the suggestion of the low Si/P preference of most *Stephanodiscus* species, and we have incorporated this into discussion. *Stephanodiscus transylvanicus* is probably hypolimnetic and mesotrophic, and actually its ecology is still unclear. It seems that relatively high *S. transylvanicus* abundance for example during the mid Holocene cannot be interpreted as strengthened and prolonged mixing, and in contrast, it is correlated with strong stratification, which is consistent with the inference of high water temperature and probably weak winds. As for the relationship between *C. fottii* and mixing, please see the reply above about the comparison of its behaviour during the Lateglacial with that during the mid Holocene.

5.4 The mid-Holocene (page 14358) Page 14358, lines 20-21. Here it is unclear how temperature would influence the abundance of the hypolimnetic Stephanodiscus transylvanicus. In general, the hypolimnion is characterized by very stable temperature conditions.

Stephanodiscus transylvanicus is probably hypolimnetic and mesotrophic, and it is possible here that this species benefits from high water temperature-induced productivity in the hypolimnion and/or high nutrient availability in the hypolimnion under strong stratification.

Interestingly, in this interval the percentages of C. fottii of large cell-size (>20 micron) are much lower than in DAZ-1. This may indicate low nutrient availability (especially silica?), so that less cells can complete their full life cycle with sexual reproduction (see Stoermer et al. 1989 as an example of down-core variations in cell-sized of a centric species). This may be linked to shorter and/or weaker circulation period.

Thank you. Please see the reply above, and we have incorporated this.

5.5 The late Holocene (pages 14359-60) Line 15: "It is not a predictable diatom response to high nutrient availability". Maybe the increase in epilimnetic C. ocellata and C.minuscula and the simultaneous decrease in hypolimnetic C. fottii are linked with a decrease in water transparency? Increased turbidity would be compatible with enhanced erosion.

As mentioned above, this lake is still highly oligotrophic and very transparent, and light limitation usually occurs in eutrophic lakes.

Lines 26-17: it is not clear here why you make a comparison with C.paraocellata from Lake Prespa. Please explain.

We have removed this kind of comparison.

Page 14360, lines 9-11: please add some reference(s) regarding the threat of invasive species.

We have incorporated the reference.

6. Conclusions As a general comment, I think that without a better estimate of diatom productivity it is difficult to evaluate how the diatom of Lake Ohrid responded to temperature variations. With that in mind, I feel that your conclusions refers too much to variations in temperature.

As mentioned above, we explore diatom reponse mainly based on diatom assembalge composition of main planktonic taxa (i.e. ecology), taking diatom concentration just as a secondary proxy. As diatom concentration data was obtained when counting according to the standard procedure of diatom analysis, we would show the concentration data here although we cannot estimate the diatom biovolume accumulation rate. In contrast to previous diatom studies in this lake, we discuss the high complexity of diatom response to Lateglacial and Holocene water temperature change in greater detail, and have also given greater weight to disscusion of the possible influences of wind forcing and catchment mediation.

Referee Keely Mills

The paper by Zhang et al. provides a diatom record from Lake Ohrid, Macedonia, spanning the last 12,300 years. In general the paper is well written, and the majority of the corrections I suggest are really quite minor in nature. The record from Lake Ohrid is unique from this location, in that such a deep lake has allowed continued sedimentation in the lake over such a long timescale. Due to the lake's depth, it is unlikely to be responding to shifts in effective moisture (as do other shallow lakes in the region) over such a timescale. This offers the chance for the authors to investigate what is driving ecological response when the precipitation signal appears to be muted. As part of this the authors focus very heavily on temperature (of what nature? Air, water, summer, winter, maximum or minimum) and temperature-dependent productivity. It is also a relief(!) to see that multiple cores from such a large lake also record the same changes!

Thank you. We have clarified that "temperature" here is "mean annual epilimnetic water temperature".

My only real criticism of the manuscript is this focus; there is no exploration of other potential drivers of change. I'm not quite convinced that temperature alone is the major driver of the changes observed in the record, and in places the discussion surrounding drivers and response (temperature-nutrients-diatoms) seems to become a little circular. Where responses differ through the record, these are then referred to as 'anomalous' or 'unpredictable', rather than exploring other mechanisms. It would be useful to see some opening up of the discussion (light, ice cover etc) and not just focus solely on a temperature and productivity interpretation.

Please see the reply above. In contrast to previous, preliminary, and low-resolution diatom studies in this lake, we discuss the high complexity of diatom response to Lateglacial and Holocene climate and environmental change with particular respect to water temperature, and we have now given greater weight to disscusion of the possible influences of both wind forcing on stratification/mixing and catchment dynamics on nutrient input, in mediating the complex response of diatoms. We include discussion of the possible influence of Lateglacial lake ice cover which is inferred from the ice-rafted debris deposition at the coring site (Wagner et al., 2012). As for the possible influence of light availability, this lake is still highly oligotrophic and exceptionally transparent (Matzinger et al., 2006a, 2007) and hypolimnetic diatoms can be found at >200 m water depth (Stanković, 1960), so light limitation can be assumed insignificant. The word "anomalous" or "unpredictable" is used to describe the diatom assemblage composition that cannot be interpreted according to the previous hypothesis (i.e. temperature-induced productivity), and surely we explore other possible mechanisms (e.g. indirect temperature influence, wind forcing, and catchment mediation) and find that diatom response is complex.

Other than this, I feel the manuscript would make a valuable contribution to the literature, and it would be of broader scientific interest to see the manuscript fully published. In addition to the major point above, I also list some minor comments (and questions for my own curiosity!) below.

Thank you.

<u>Specific comments</u>

Introduction

Needs some context in terms of why this region is a key site in the north eastern Mediterranean region perhaps? What is it that is contentious or interesting in other records that exist (outside of Lake Ohrid)? What are the other records showing over this time frame to provide some kind of climatic context - this would then link aspects of the discussion (e.g. MIS 2, ice rafted debris, 8.2 event, 4.2 event, Mediaeval Climate anomaly etc) to the information in the introduction. This can then lead in to the specifics of previous work undertaken on Lake Ohrid, and you're your aims/objectives for this paper.

We reviewed in another paper that the southern Balkan region is located at the juncture between the west–east and north–south contrasting Holocene hydroclimatic domains (Zhang et al., 2014), and moisture availability and lake-level change is the main topic in Mediterranean palaeoclimate research. We would not repeat this and more importantly this study does not contribute to understanding of Holocene moisture change. In contrast, temperature reconstructions in the northeastern Mediterranean region are rare, and due to the unique (deep and ancient) character of Lake Ohird in this region, it is an excellent opportunity to investigate Holocene temperature change in this study. So we have incorporated a paragraph to discuss existing temperature reconstructions in this region and their potential problems in the section "Introduction".

Site description

14347 Ln3 Is the >1.2 Ma sedimentary record continuous? If so, can you state this – I think this is amazing!

We have clarified that it is continuous.

14347 Ln21 I just wonder what quantities the percentages refer to, and over what time frame? Is this the average over 1 year, 10 years etc? What is the volume of water that totals 100%? 10L, 1000GL? It's difficult to get a perspective of how much water is moving through the system without knowing the volumes. What's the lake water residence time, can this influence the internal nutrient cycling?

Water balance consists of both input and output components. Inputs to Lake Ohrid include river inflows, precipitation on lake surface, surface springs and sublacustrine springs; outputs include river outflow and evaporation. River outflow rate ($m^3\ s^{-1}$) is obtained based on regular measurements and then calculated to the amount of water outflow in one year ($m^3\ yr^{-1}$), and evaporation ($mm\ yr^{-1}$) is estimated through the Penman approach and then calculated to the amount of water evaporated in one year ($m^3\ yr^{-1}$) by multiplying the surface area of the lake. These two output components combine as the total volume of water. Similarly, the water amount of river inflows, precipitation and surface springs can be calculated, respectively. The water amount from sublacustrine springs is estimated from closing the balance of the total water volume in one year. So the percentages of each component can be obtained through being divided by the total water volume in one year (Watzin et al., 2002; Matzinger et al., 2006a). As the section "Site description" is already quite long, we just incorporate the results in it. Lake water residence time is about 70 years and complete lake circulation occurs about every seven years (Matzinger et al., 2006a), and we take internal nutrient cycling into discussion.

14348 Ln5/6 What is the driver of the 7 year complete turnover – the return interval of severe storms (which I think you allude to in the discussion)? A brief mention of the mechanism would be useful/interesting.

Occasional complete lake circulation usually occurs in severe winters or following intense wind action in less severe winters in this lake today (Stanković, 1960), and thus in-lake mixing-induced nutrient regeneration could be driven by epilimnetic water cooling and wind forcing. We have incorporated this into discussion.

14348 Ln11 You give the 'mean' TP and TN values throughout the water column in the centre of the lake, but then proceed to provide a range (not a mean?). How does this vary through the profile? Where are the highest concentrations? When do the highest concentrations occur (in epilimnion in autumn for example) – could easily be represented in graph form? How typical was 2000-2001 of longer term concentrations (I appreciate these data might not exist – just curious!).

We have deleted the word "mean", and the range means the variation in monthly values. Watzin et al. (2002) only carried out measurements in the year 2000-2001 (all the dates placed in brackets now), and there is no clear seasonality in these monthly data.

Material(s) and methods

14348 Ln 22 Replace ‘Not taking into account’ with ‘Excluding…’

We have modified this.

14348 Ln 24 Replace ‘revealed’ with ‘identified’

We have modified this.

14349 Ln6 Why did you use the 0.1 smooth – what happened to age model with other smooth values – how did it affect the age-depth relationship, and associated errors?

It proves that using smoothing=0.1 provides the best fit of the age model to all age control points. Changing the settings or even more the interpolation method (e.g. stepping back to linear interpolation when smoothing=1) of course changes the model, and it just has to be figured out what describes the data best.

14349 Ln7/8 Not sure it is correct to say ‘radiocarbon age of fish remains is apparently too old’ without qualifying? It’s clear that in conjunction with multiple dated horizons either side, it appeared there was an issue with the material dated, but you kind of dismiss it out of hand in the text? Why did you get an odd date for this which is older (I always find it easier to explain erroneously younger dates!)?

We have clarified that the fish remains is probably affected by a reservior effect or that they are redeposited (Wagner et al., 2012).

14349 Ln13 You really cannot claim this is a high resolution record. Not in terms of sampling (104 samples out of 785 cm) nor in terms of age intervals (ranges from 40 years to 350 years), if your samples are 8 cm apart, the resolution of record is quite low temporally. Remove the phrase and just stick with the actual values already in text.

We have deleted the phrase “high resolution”.

14350 Ln 21 It would be much better to phrase as ‘…diatom preservation, the F index of Ryves et al (2001) was used to calculate the dissolution of the dominant endemic taxon…’

We have modified this.

14350 Ln21 Did you use the four stage approach of Ryves? If so, would be nice to include photos in a supplement to see how the dissolution changed (even though it actually looks pretty good throughout the core?!)

We just simply separated diatoms into pristine and partially-dissolved valves to calculate the F index, rather than using the four stage approach to calculate the diatom dissolution index (DDI).

14350 Ln24 Ordination techniques, maybe just include a line or two of additional detail? Did you / how did you transform your count data? Square root or percentage? DCA didn’t give the largest gradient length of 1.85, you used DCA to calculate the gradient length so that you could decide whether to use a linear or unimodal ordination?

We transformed the counting data into percentages. The largest gradient length of initial detrended correspondence analysis (DCA) was considered when deciding whether to use the linear or unimodal ordination method. If the longest gradient is shorter than about two, principal components analysis (PCA) (a linear method) is appropriate; if this value is larger than about four, DCA (a unimodal method) is appropriate; and if it is about three, DCA and PCA may result in similar configurations (Ter Braak, 1995; Lepš and Šmilauer, 2003). In this study PCA was thus selected.

14350 Ln 29 ‘…the influence of sedimentation rate was factored out…’, I’m not sure what you mean here, as in you corrected for sedimentation rate?

We have reworded this sentence.

Results

From this point onwards, you have capitalised words such as 'Axis' and 'Subzone' and 'Zone', I don't think there is any need for this. Can these be changed to lower case?

Our wider research group always capitalise the first letter of these words, so we are following their protocol.

14351 Ln8 This sentence reads a little muddled. I think some punctuation is needed? 'F values for endemic are >0.75 throughout the record'. What does the >500 valves refer to, or are you just stating that's what you counted? Are you saying that concentrations were generally high (exceeding 2×10^7 *valves*?? g-1) and you've already made the point that preservation was good. Just rephrase this section to make more sense.

Thank you. We have modified to clarify all of these.

14351 Ln21 *S. transylvanicus* 'occurs' as in it appears for the first time? Maybe rephrase? What reaches peak diatom concentration in this zone? Clarify, I'm not sure what you're getting at.

Stephanodiscus transylvanicus appears for the first time, and we have modified to clarify these.

14352 Ln3 'by **an** increased…'

We have modified this.

Interpretation

Just out of interest, is there not a temperature record from this region to which you could directly compare your data? If there is, it would certainly help to support you temperature interpretation?

As mentioned above, we have incorporated a paragraph in the section "Introduction" to discuss existing temperature reconstructions in this region and their potential problems. To strengthen interpretation, we compare diatom data mainly with the sedimentological, geochemical and palynological data from the same lake. There is not such a temperature record in this region that can be compared directly with our data, and we have compared particularly with the globally-stacked proxy mean annual surface temperature record (Shakun et al., 2012; Marcott et al., 2013).

14353 Ln7/11 You choose an interpretation 'in contrast' to other studies? Why? What is the reason for this? Can you expand a little?

Diatoms have been only interpreted as temperature-induced lake productivity in previous studies on the last glacial-interglacial cycle (cf. Wagner et al., 2009; Reed et al., 2010). This study focuses on the Lateglacial and Holocene period to test the response of diatoms in greater detail, and also has incorporated the possible influences of wind forcing and catchment mediation. We find that diatom response is complex and not straightforward as in previous studies. The high complexity of diatom response compared to the conclusions drawn in the longer timescale but lower-resolution analyses is indeed the most important finding of this study. The phrase "in contrast" is used to strengthen the comparison and draw much attention to the finding of this study.

14353 Ln9 Low organic matter may be reflecting nature of the catchment, if poorly vegetated, more clastic input might dilute algal signal? This might easily be seen if the diatom data were converted to flux data – algal input might not change, just the quantity/nature of the terrestrial material?

We agree with you that clastic input may dilute the signal of algal production as mentioned above. Diatom flux data can rule out the influence of sedimentation rate, and we assess qualitatively the possible influence of sedimentation rate, since we cannot calculate the flux data due to the lack of the necessary dry sediment bulk density data. We have also considered sedimentation rate, K intensity (i.e. clastic content) and calcite and organic matter content (i.e. authigenic matter content) together to assess the strength of catchment erosion qualitatively.

14353 Ln16 is 'putative the right word to use here?

We have removed this word.

14353 Ln15 The sentence starting Diatom PCA axis 1 scores…' is very long and confusing. I suspect there is a word or two missing around the phrase '…high positive scores **are coincident** with the dominance of epilimnetic taxa…' Please clarify.

Thank you. We have clarified this.

14354 Ln11 Two problematic taxa S. pinnata and P. brevistriata (I am no fan of these two)! No clear habitat preference, usually a sign of instability/fluctuating conditions in a lake system (kind of weedy species the world over…)

We have modified this sentence, incorporating their sign of instability into discussion.

14354 Ln 20 'Thus, erosion-induced external nutrient input would also have been high?' Not necessarily? Would be really good to bring in/refer to the palynological record here, it might be a poorly vegetated catchment with few pioneer species, which require little in the way of nutrients/nitrogen (as they may fix their own) – this would mean a nutrient poor catchment, so even if material input from catchment was high, it may not have been nutrient rich? Which then leads to…if increased input is coming from the catchment (nutrient rich or not), what does this do to the **turbidity** of the system and **light availability**, and how does this affect the diatoms? And, I guess if it's cold, what role does (extended/permanent?) **ice cover** have to play?

Catchment erosion was high during this period, as suggested by high sedimentation rate, high K intensity (i.e. high clastic content), and low calcite and organic matter content (i.e. low authigenic matter content). This is supported by sparse vegetation and unsettled soils in the catchment (Panagiotopoulos et al., 2013). We bring in discussion of catchment vagetation here to consider the function of vegetation on retaining the soils. Although the soils formed during this period were poor in nutrients, it is possible that the soils formed previously can also be eroded into the lake without the retention of overlying vegetation. The influence of turbidity and light limitation on diatom growth usually dominates in eutrophic lakes, but this lake is still highly oligotrophic and exceptionally transparent (Matzinger et al., 2006a, 2007), and thus light limitation can be assumed insignificant. In terms of ice cover, we discuss its influence on changes of facultative planktonic species.

14355 Ln4 If temperature is increasing diversity of samples too, then it wouldn't just be diatom diversity increasing? How would the response of increased temperatures on **zooplankton/grazers** work in favour of selection for smaller cell-sizes in diatoms? Can **increased competition** also favour smaller cell sizes? What about **stability in stratification**? **Changes in wind regime** if catchment vegetation increases etc (again references to palynological record useful here too)? These kinds of issues are pertinent to and could be raised through the rest of the discussion too!

The diversity of planktonic diatoms in Lake Ohrid is actually quite low, and our data show no change in diatom diversity associated with Holocene water temperature change. Although zooplankton grazing and interspecific competition are probably potential factors, we just focuses on climatic, environmental and limnological influence on the development of small-sized diatom species in this manuscript. As mentioned above, we have given greater weight to discussion of the possible influences of wind forcing and catchment processes in mediating the complex response of diatoms.

14355 Ln17 Is it not unsurprising that the response is muted in Ohrid? It's a *really* deep lake! Other lakes you mention are much shallower, so their response would be more amplified? **Just in general**, what role does catchment size and groundwater play in mediating any responses observed in the lake? Do you have any lake water balance modelling?

The muted water temperature increase during the Lateglacial-Holocene transition in Lake Ohrid is very surprising, and this is not consistent with a distinctly increasing trend for the globally-stacked proxy mean annual surface temperature (Shakun et al., 2012; Marcott et al., 2013). It is still unclear why this happened. The strong response of shallower Mediterranean lakes is a signature of marked increase in precipitation rather than temperature at the beginning of the Holocene. Water balance modelling shows that 53% of water input is from karstic springs (Matzinger et al., 2006a); however, as mentioned above, due to the coring location of core Co1262 far from spring areas, the direct influence of springs is probably negligible in terms of nutrient transport. From preliminary comparison of Lake Ohrid with shallower Lakes Dojran, Prespa and Ioannina, it seems that different diatom responses relate to water depth rather than catchment size.

14555 Ln27 Yes, certainly potential – assuming we have a good grasp of the functioning and response of the modern system. I suspect this is where the geochemistry work of Lacey et al will really help in the long run.

We will prepare a separate manuscript to discuss the different responses of diatoms in the lakes of different types to Holocene climate and environmental change.

14356 In section 5.3, you often refer to 'high' temperatures. What kind of shift in temperature are we talking about? And what is the nature of this temperature? Air? Surface Water? Mean annual? Summer max etc. Need to clarify and may be avoid the use of high (at worst case, replace with higher; best case quantify or use alternative description altogether).

As mentioned above, we have clarified that "temperature" here is "mean annual epilimnetic water temperature", in abbrevation "water temperature". Our proxy data do not allow to quantify the temperature reconstruction, and just allow to assess "high" or "low" qualitatively. Since the most recent section (e.g. the late Holocene) of the record is seriously affected by human impact, "higher" or "lower" temperature than today is also difficult to assess.

14356 Ln 11 Again, probably a more **general comment** for consideration, spurred by this section. If productivity is causing centric species to bloom, won't these also affect light availability, reducing light to species that prefer the hypolimnion? How would this interplay affect the abundance of the various species?

As mentioned above, Lake Ohrid is still highly oligotrophic and exceptionally transparent (see "Site description"; Matzinger et al., 2006a, 2007) and hypolimnetic diatoms can be found at >200 m water depth (Stanković, 1960), so light limitation can be assumed insignificant.

14357 With regards to the tephra, can you really make the conclusions you do given the resolution of your record? Assuming your sample resolution is at best 40 years per sample or worst 350 years per sample (or scaling up at every 4 cm interval 160-1400 years or at every 8 cm interval 320-2800 years). First 'Mercato tephra' (Ln12) if the impact/response is short-lived it would be highly unlikely that your record would capture it, so it's probably not fair to say it had no impact? Your sampling just didn't pick it up – or your integrated sample was just that, and couldn't differentiate? Could the same be said for the 8.2 ka event? Or are diatoms not completely driven by temperature changes? On page 14358 Ln24 could the same apply to the 4.2 event?

We agree with you that analytical resolution is important for discussing the tephra impact. We talk about the sample interval and age resolution of diatom analysis in the section "Material and methods", and the age resolution of diatom samples around the Mercato tephra is about 100 years. It is possible that our diatom record does not capture it as you pointed out, but here we discuss its possbile impact on diatoms, that is, according to the typical characteristics of tephra impact on diatoms, we can rule out its influence on the changes in diatom composition and concentration in this study. The putative 8.2 ka event is clearly indicated by geochemical proxy data from the same sequence in this study, covering ca. 600 years, and we improved the sample resolution of diatom analysis to 4 cm apart (ca. 100-year resolution) to test its possible influence on diatoms. The diatoms show no response to this climatic shift, and we have clarified this in the revised manuscript. The discussions of diatom response to the 4.2 ka event and MCA (as you pointed out below) are similar, and diatoms show no response too.

14358 Ln6 What is 'high' for temperature?

It is the maximum for water temperature during the Holocene, strongly indicated by high calcite content. We have clarified this.

14358 Ln8 Just because it doesn't fit a 'temperature' explanation, doesn't make the response anomalous. It means that something other than/as well as temperature is really driving the diatom response, and that it is totally plausible for the factors to shift through time.

It just does not fit the previous hypothesis of temperature-induced lake productivity. We have clarified that variations in the relative abundance of main planktonic diatom taxa may be a direct response to shifts in temperature-induced lake productivity, and we should also consider the possible influences of temperature-related stratification/mixing regime, wind forcing, catchment mediation, light limitation, and/or spring inflow. As mentioned above, the possible influences of light limitation and spring inflow on diatoms at the site of core Co1262 can be ignored, and we have given greater weight to discussion of the possible influences of wind forcing on stratification/mixing and of catchment dynamics on nutrient input.

14359 Ln9 What is this human activity you speak of?! Can you qualify what the impact might be? Catchment clearance? Atmospheric N deposition, agriculture, groundwater abstraction etc.

We have clarified that it is probably mainly forest clearance and agricultural development, although it is difficult to qualify it in this study.

14359 Ln15 Not sure of the use of the word 'predictable' please change – a response largely depends upon many things (antecedent conditions, internal thresholds, buffer etc), so may not be 'predictable' at all…

We have modified this sentence and deleted the word "predictable".

14360 Ln4 Mediaeval Climate Anomaly (not MWP) – again, sample/study resolution may play a factor in this.

Thank you. We have changed Medieval Warm Period (MWP) to Medieval Climate Anomaly (MCA). As for sample resolution, please see the reply above.

Conclusions

Will need modification in response to changes in the discussion.

We have modified accordingly.

Figures

Figure 3 & 4 Does axis label for diatom concentration need to state if it is valves or frustules?

We have clarified that it is valve concentration through having modified the unit to "10^7 valves g^{-1}".

Figure 5 Shows the dominant species from 2 cores, can the species be placed in the same order (where they are similar) just to make it a little quicker/easier for the reader to see.

We have modified to show the dominant species in the same order.