

Dear editors,
please find herewith our reply to the three comments to MS No. bg-2010-156 entitled:

Carbonate sedimentation and effects of eutrophication observed at the Kališta subaquatic springs in Lake Ohrid (Macedonia)

by M. Matter, F.S. Anselmetti, B. Jordanoska, B. Wagner, M. Wessels and A. Wüest

submitted to the Special Issue: Evolutionary and geological history of Balkan lakes Ohrid and Prespa

We appreciated the constructive comments of the 3 reviewers (all flagged as minor revisions) and incorporated most suggested changes in a revised manuscript, which consequently improved significantly. Please find below our responses (in green text) to all raised points of the reviewers (in black text).

Two main issues were often discussed in the reviewer's comments, which we comment upfront (A, B) and to which we refer then in the detailed remarks as well (rather than repeating our comments and changes each time these issues are raised by the reviewers).

A) Age model

We realize that the age data does not provide the means to define an accurate age model spanning the entire stratigraphy. Our Cs-data nevertheless show clearly that the Unit T-Unit L boundary separates pre-1955 from post-1955 sediments. The inconsistent radiocarbon and non-complete Cs and Pb profiles do confirm the dominance of a very dynamic sedimentation/erosion system. We already recognized this in the first manuscript version, as in the age model section (4.3) this was described in this fashion. We realize, that we somehow overinterpret the data by assigning sedimentation rates to the Unit T deposits, as the age of the base of Unit T is likely locally varying (albeit post 1955) and sedimentation is interrupted by sediment reworking periods or events. Section 5.2 in the discussion section was thus modified, so that these sedimentation/erosion processes are highlighted, and that not absolute sedimentation rates are calculated anymore for the post 1955 section. We now just compare Unit T thickness, and observe a pattern with enhanced values near the springs and lower values at larger distances to the spring. As this pattern seems to be systematic, we believe that we can say that the springs provide an enhanced particle source through favouring carbonate precipitation. We will, however, not quantify this effect and leave it with this relative statement.

B) Eutrophication

The key observations that lead us to conclude that we indeed see a eutrophication signal across the Unit L-Unit T boundary is the upcore change towards darker sediment color, an increase in TOC, a slight decrease in carbonate, an increase in Fe and Si content as well as in diatom content, and a sewage smell upon core opening. These observations are all compatible with generally enhanced supply of nutrients to the lake. There are, however, no data on water chemistry changes through time, which would quantify this eutrophication signal. In the revised version, we made this now more clear and added a comparison to the deeper water cores. There, a coincident change, mostly expressed as a drop in carbonate content (Fig. 9) also occurs that might have been caused by the same processes. As longterm water chemistry data are not available, we simply conclude now that eutrophication likely is a lake-wide phenomena, which becomes enhanced and more sensitively registered in the shallow-water areas.

Reviewer 1

General Comments:

This paper is an interesting study on the role of subaquatic springs in precipitation of

authigenic carbonate sediment in Lake Ohrid. Lake Ohrid is targeted for the deep drilling in ICDP and, so, detailed knowledge of the role of the springs is relevant to a better understanding of deposition of lake sediments and the paleoenvironmental significance of sedimentological changes throughout the lake cores. The study concludes that the subaquatic springs influence sedimentation locally, but not widely across the lake. Accumulation rates are significantly higher in the spring area. The data and methods are generally robust and the conclusions seem, in general, well substantiated. The images of the precipitated calcite and the proposed role of pico- cyanobacteria add more data to our understanding of microbes in authigenic calcite precipitation. My concerns are that there are significant problems with the age model that and not adequately discussed.
+ (see comments in A above).

I also recommend more comparison with shallow areas of the lake that aren't influenced by springs.

+ We agree that this comparison is necessary for an evaluation of the effects of the springs. The design of our coring transect somehow allows such a comparison, as the northern transect moves more than 1 km away from the spring area. This provides some clues, how the local spring hydrology does affect the sedimentation pattern. We do not have other data at hand, that would provide shallow water sedimentation from farther away but we believe that at a km-distance, the effect of the spring should be minimal as the waters are strongly diluted.

Specific Comments

Lake sediment core studies are inherently constrained by the ability to date the sediments. This study is heavily so - and not for lack of effort. Although I don't disagree that pre and post- 1955 is a useful distinction for Unit L-T boundary, I do think that the authors should be more upfront about the possibility that the sediments are very highly bioturbated and that there is significant local movement of the sediment that makes estimation of accumulation rates very very difficult to even approximate. It is highly speculative to say anything about accumulation rates in the sediments - particularly in the Spring zone. For example, it's surprising to be that Unit T should vary in thickness between 10 cm and 28 cm within a rather small zone. There might be some useful information in more careful thinking about why dating techniques don't seem to work in these sediment cores. For example, is there a reason that the charcoal ^{14}C dates, while unrealistically old, are consistent with each other? Without a robust age model, sediment accumulation rates in the cores remain very highly speculative. Therefore, I wonder if figure 4, which details accumulation rates in the water column, adds information to this study. I don't think it's necessary.

+ see comments about age model in section A above.

Rather than for the age model, Fig. 4 provides information in terms of quantitative water column processes and thus documents how much particles are descending in the water column, mostly related to authigenic processes. Furthermore, it also shows high accumulation rates compared to the sediment trap data from the basinal locations (Matzinger et al., 2006b). Whether bioturbation or 'local movement' impact the sedimentation rate is not the focus of this figure, and we thus prefer to keep it in the manuscript.

The authors provide core data from one deeper water site, 20, that don't indicate a Unit T. Is this consistent with other cores from deeper parts of the lake?

+ This is correct, as most deeper water cores do not show such a clear lithologic boundary coinciding with the Unit T-Unit L boundary observed in the shallow-water cores. We made this clearer in the manuscript and also added once more in section 5.2. a reference to a study that documented the deeper-water lithologies (Matzinger et al., 2006b).

Do the authors conclude that nutrient loading (eutrophication) is a shallow water condition?

+ see comments to eutrophication in section B above. We conclude that it is a lake-wide

phenomena, which becomes enhanced and more sensitively registered in the shallow-water areas.

The decreasing trend of TIC values in most cores (Figure 9) could be developed and discussed more completely in the text. Interpreting wt % data can be compounding by a the variety of factors that would influence wt %. For example - TIC % would decrease as a function of a OC% increase – all other constituents being constant. Because this TIC decrease is seen in most cores – independent of the water depth – could this be indication of lake-wide eutrophication. The authors might consider revising their wording on eutrophication in the introduction (pg. 4718 lines 20-25) to indicate more strongly the possibility for eutrophication (which is discussed throughout the paper) and to indicate the thresholds between oligotrophic and eutrophic conditions.

+ see comments to eutrophication in section B above.

It would be interesting to see some data from sediment trap and core data from other shallow areas of the lake which are completely outside of the influence of the subaqueous springs – to really dissect the influence of the springs.

+ We agree that a comparison with other sediment trap data from the shallow areas would be very interesting. However, no further shallow-water sediment trap data were collected and only trap data from the deeper area were collected within another study (Matzinger et al, 2006b).

Table 1. The text suggests that there is data since 2004, and the data suggests that there is a significant seasonal component. Although I realize that carbonate precipitation is a summer phenomena, is there more water chemistry data to include that would fill out the season? Also, it would be useful to compare with late water chemistry data from an area outside of the spring zone.

+ It is a good suggestion to compare the data with data from deeper cores. We already state in the discussion section that sediment trap data from the deeper water (Matzinger et al, 2006b) showed lower TIC values than in the spring area (7.8% vs. 8-5-10.4%). We now completed this statement by adding that sedimentation rates are also lower (~0.9 mm/yr) and that our Ca concentrations at the spring zone (21-34 mg/l) are also higher than reported average lake wide values of 13 mg/l.

We, however, cannot provide any seasonal time series of water chemistry data, as they are not available.

The careful SEM documentation of the calcite crystals and description of the idiomorphic crystals and their clusters from the sediment traps adds to our understanding of the role of microbes in calcite precipitation. It would be interesting to have pictures from calcite crystals in the cores from the deeper part of the lake and also shallow part with that are outside the influence of the subaquatic springs to compare. Also, further development of the differences in the crystal shapes between Unit L and Unit T could be added to the discussion.

+ There are some SEM images from basinal cores (Drya, unpubl.) that show similar features in respect to the calcite crystals. An initial comparison between Unit T and Unit L calcite crystals did not reveal significant changes in nature of calcite crystals, as the process (but not the rate) leading to authigenic calcite precipitation likely is the same.

Technical comments

Table 1 – Formatting for the first date, April 3, 2007, is not consistent with the rest of the formatting for dates in the table.

+ Not quite clear what is meant here, all formats are the same.

Also the Ca²⁺ [i A M] data from 27 September 2007 1m looks inconsistent with the reported mg/l concentrations. Is this a typing error?

+ There were indeed some errors in the second column of the Table. They have been corrected.

Pg 4722 line 4 – I suggest you change “in the lowest trap of mooring 2” to “in the deepest trap of mooring 2”.

+ Has been changed

Figure 6. Why are pictures of the cores from the Eastern Transect not included?

+ The cores shown in Figure 6 already are plotted at small scale, so that we opted no to show in this figure the cores also from the eastern transect. The core photos of these cores, however, are shown in Figure 7, we mention this now in the figure caption of Fig. 6.

Figure 7. This figure should be enlarged. It's difficult to see the information.

+ We already stretched the figure to the maximum page width and prefer not to take the figure apart, as intercore comparison can better done when the cores are shown side-by-side.

Cores 12 and 13 seem to be mislabeled as Southern Transect (where they are positioned on the Northern transect in Figure 2).

+ Good remark, has been changed.

It seems inconsistent to include pictures of the Southern transect cores (but not geochemical data), but geochemical data from the Eastern transect (without presenting the pictures).

+ The geochemical data from the northern and southern transect are very similar and showing the data from the south does not provide new insights. In contrast, the eastern transect reaches deeper water and provides a link from this study to previous basinal studies, this is why we think our choice of data selection is justified. As mentioned above, Fig. 7 does already show the core photographs of the two eastern transect cores, reviewer 1 must have overlooked this.

Reviewer 2

General Comments.

The paper by M. Matter and collaborators on the carbonate sedimentation at the Kalista spring area in Lake Ohrid (Macedonia and Albania) provides new and interesting data on recent carbonate formation in lacustrine littoral settings with high spring input. The methodology is sound and included water chemistry, sediment traps, transects of short cores, and side-scan imaging. The study site is extremely interesting from a sedimentological, ecological and societal point of view (the oldest lake in Europe, a target for ICDP project, a large water reservoir). Two major conclusions of the paper are that calcite formation due to spring activity is a major contribution of littoral carbonate productivity (and not so much to distal areas) and that recent changes in sedimentation patterns are likely caused by eutrophication and human impact. The variety of calcite crystal morphologies and the likely relation with cyanobacteria is well documented in the paper. Although is not the focus of the study I miss a more detailed description of the sedimentary facies and the depositional processes that could help to understand better the dynamics of the littoral environments. The comparison with previous sediment cores studies in the lake (although from distal zones) could also help to pinpoint their main features of spring-affected versus non-spring littoral zones.

+ We appreciate these comments and agree that a comparison with other cores would be beneficial. We added thus more comparisons to the deeper-water cores from Matzinger et al., 2006b (explained above), however, no comparable littoral cores are available. There are cores from a subaquatic terrasse at water depths of 30 m and deeper (Lindhorst et al, same issue), but they offer no comparison, as they are in a much lower-energy environment and furthermore have totally different lithologies as Late Glacial deposits occur already in few cm depth.

The hypothesis of eutrophication as the main cause for changes in littoral sedimentation

in the last 50 years will need further testing.
+ see comments about eutrophication in section B above.

Detailed comments

Introduction

Water chemistry data seem to be available since 2004 but only three surveys from 2007 are included in table 1. Is there any information on seasonal changes in water chemistry of the springs?. Those could be interesting to test if maximum carbonate productivity associated to bacterial activity (summer) is also related to changes in chemical composition.

+ This is a good suggestion, however, no additional spring-water chemistry data from other seasons are available. They might be collected with upcoming campaigns with our partners at the lake station, however, they will not be available for this manuscript.

Changes in carbonate productivity during the Holocene have occurred in the distal areas of Lake Ohrid, which suggest lower ion concentration (dilution) or decreased spring activity. Although the time scale of these changes is centennial or decadal, the same mechanisms could play a role in changes during the last 50 years in littoral zones. Any data on decadal changes of spring activity in the area?. One of the goals of the paper is to investigate whether spring sedimentation changed through time, although the uncertainties of the age model hampered clear conclusions.

+ These data also would be very nice to have indeed, but no decadal-scale measurements have been performed in the past.

Methods.

The methodology is sound and very comprehensive. Grain size analyses are mentioned but I guess they are used mostly to define the facies since no grain size profiles are included in the paper.

+ That is correct. We did measure grain sizes in order to get a idea on sediment properties and general characteristics. The grain-size curves, however, do not provide critical information that would substantially contribute to the here presented study, so that we opted not to show them, also streamlining the manuscript.

Results

Sediment traps

I wonder why that particular period in May was chosen for the deployment of the sediment traps. Most carbonate formation in the lake epilimnion seems to happen in summer and it would have been interesting to be able to compare littoral sediment traps with distal sediment traps during the same periods. Are they confident that May data are representative of maximum carbonate productivity?.

+ The choice of performing the survey in May was partly caused by logistic considerations (availability of boat and team). However, early summer is considered to be the main kick-off for biologic productivity, as water temperatures steeply rise. We consider this period to be closely as productive as high summer, a pattern also confirmed by the basinal sediment trap data of Matzinger et al., 2006b.

How those sediment rates and fluxes compare with distal areas?. A more detailed comparison of the SEM/EDX data with a similar study of distal carbonates could also help to evaluate the contribution of littoral, spring-generated carbonates to distal areas in the lake (that is likely very small).

+ As discussed already above, we compare now our geochemistry and sedimentation rate data with the values from cores and traps reported in Matzinger et al (2006b), who showed lower values in the deeper water. This confirms the elevated rates of carbonate precipitation in the shallow-water spring area. Only few SEM data from previous studies are available, showing somehow similar crystal morphologies.

The pictures are pretty convincing of cyanobacterial activity as the main origin for some crystals. However, since macrophyte patches are common in the spring area I wonder if there are also any charophytes or some of the macrophytes could become calcite-encrusted and also contribute to carbonate sedimentation in the littoral zone.

+ This could well be the case, but we did not see substantial particle contributions from charophyte-type organisms. We prefer not to elaborate on this, because this would be difficult to quantify and because cyanobacterial activities (as depicted by SEM morphology) seems to be clearly the dominant process, thus relevant for the bulk of the carbonate sediment.

Sediment Cores Lithologies

Short cores show a large depositional variability in the littoral zone and even more in the spring area. The side scan sonar also illustrates the presence of macrophyte patches, bedrock, spring and sediment covered surfaces. This is common in lakes and to be expected in Lake Ohrid. Although the lithology is not very diverse, more details could be given in the facies. For example, there is an indication of “coarser layers” with lower carbonate content (and higher Fe counts) and also gravel beds in the lower part of some of the cores. Have those layers more siliciclastic materials?, could they represent input from run off?.

+ This is a good remark, the coarser layers indeed show lower carbonate contents (as stated in the description of the lithologies), which we interpret to be related to supply of detrital material from strong runoff events. We added this phrase in the discussion chapter: 'The lower carbonate content in the few coarse layers in Unit L is interpreted as a supply also from non-carbonate detrital material, that originates from strong runoff events'.

They seem to be only in unit L, which would suggest a different (more energetic) depositional environment. However, in core 13, the lower part of unit T has a similar color (lighter) and increase in Fe as one of those coarser layers (around 15-18 cm). So I wonder if currents, wave and reworking processes are of significant importance in the upper unit T, too.

+ The fact that coarse layers are more abundant in Unit L could also be caused by the fact that that this unit represents much more time, so it is more prone to be affected by wave-base lowerings winnowing the fine particles. The mentioned lower part of Unit T actually is characterized by the common geochemical pattern seen in most parts of Unit T. Furthermore, Fe is difficult to interpret in Lake Ohrid as it is not only terrigenous but also diagenetic as concretions may occur, in particular in lithologies with high TOC (Wagner et al., same issue, Vogel et al., 2009). We added this thought in the discussion section (Paleoenvironmental history).

This is important since these processes could be more significant in total sedimentation rate than just calcite precipitation from springs. I think it would be useful to better characterize the different facies in the cores, at least the upper unit T.

+ We improved the lithologic description of both units by providing more information on components and sediment structures in Chapter 4.2.1. We also refer to Matter (2007) who provided 7 and 4 lithotypes for Unit T and L, respectively, on the basis of constituents, grain size, color and geochemical signatures. We opted not to show this detailed lithologic logs as it does not contribute significantly to the general story of the manuscript.

Core 20 is clearly different and the carbonate range is very large (9 – 67%). It is difficult to see these differences in Figure 7 and a change in scale could help.

+ Core 20 shows indeed a different lithology compared to all other cores from more shallow waters. It forms the transition with all proxies to the basinal cores, as stated in the manuscript. We appreciate the comment on enlarging the plots or scales, but as mentioned above, this figure is already at maximum allowed width.

Geochemical signatures.

Si seems to be mostly related to diatom content, but smear slides could help to check for the presence of clay minerals or other silicates.

This is correct, and diatoms have been observed in rather large number in Unit T. As clay minerals are, however, are not easily discernable in the smear slides (and as only few siliciclastic larger grains have been observed), we cannot exclude that part of the signal originates from siliciclastic constituents. The large number of diatoms nevertheless support the interpretation that the bulk of the Si-signal originates from this biogenic component.

What is the reason for the increase in Fe in the upper unit?. Increase sediment delivery to the littoral zone related to run off ?, iron oxides?. A similar increase occurred in core 13 associated to coarser material, so the mechanisms could be similar. A few mineralogical observations (smear slides) or DRX analyses could help.

+ Our performed smear slide analysis did not reveal the clear nature of the Fe-carrying particles but we can well imagine that it probably is a runoff-related signal. Alternatively, however, it could be a postdepositional process (FeS). As we are not sure and have no data we added this sentence in Section 5.3:

'...The general increase in Fe in Unit T is difficult to interpret as Fe is in Lake Ohrid not only of terrigenous but also of diagenetic origin, as concretions may occur, in particular in lithologies with high TOC (Wagner et al., same issue, Vogel et al., 2010)...'

Particles.

Is core 20 representative of distal environments of the lake?. Most of calcite crystals in core 20 come from littoral areas by downslope processes?. How about direct calcite precipitation in the epilimnion?. Diatoms seem to be almost absent in unit L and dissolution of the frustules should be taken into consideration. In some alkaline lakes, they can be present in recent sediments, but disappear deeper in the core. Changes in past alkalinity conditions during deposition of units T and L could also have an impact on diatom preservation. Higher diatom productivity could be related to increase nutrient and eutrophication, but other possibilities should be explored. I understand this requires a diatom study and this is out of the main focus of the paper.

+ Yes, core 20 is representative of deeper water lithologies, and we made this more clear in the revised version (see comments above). The raised issues and questions by Reviewer 2 are surely significant, but as this study focuses on the shallow-water environment and uses the deeper-water core only for a general comparison, we prefer not to go in these details, as they also would require totally new analysis lengthen and broadening the manuscript dramatically.

Core correlations

Lithological units are not easy to correlate among the cores and they show a large variability. Unit T is thicker in some of the sites in the spring zone, (5) but also in the transitional cores 14 and 8. The correlation with the distal cores is even more difficult. The increase in organic matter in core 20 (unless it is at about 50 cm depth) is difficult to point. In any case, I think showing this variability is an interesting aspect of the paper. How do these cores relate to other cores taken in the lake?. I think this section could be expanded a little bit, including a short comparison with distal cores taken in previous campaigns and already published.

+ We expanded a bit the section a little bit and state that the trend seen in core 20 is indeed representative for the area, matching also a comment of Reviewer 1. We clarified this in the manuscript by adding in section 5.2. a reference to a study that documented the deeper-water lithologies (Matzinger et al., 2006b).

Age Model

The authors have tried to come up with a good age model for the sediments and this is another example of how difficult is to date littoral lacustrine cores. I agree the 1955 is a good basal date for unit T and most likely an erosional or unconformity surface is the limit with unit L. Since it is not clear the nature of the boundary the age of the basal sediments of unit T could be different in the different locations, accounting for some of

the differences in sediment thickness. As the authors state, sedimentation in upper unit T could be non-continuous and other processes (reworking, waves, currents, littoral transport) could be in place. All these uncertainties make very difficult to calculate sedimentation rates. I still believe the exercise is worth it, but not much should be made of the numbers obtained.

+ We agree and incorporated these thoughts in the revised version (see comments on age model changes in A above).

Discussion

The fact that cyanobacteria seem to be the most likely candidate for biological enhancement of calcite precipitation is a significant finding of the paper. Even with the age uncertainties, the higher sedimentation rate in littoral spring zones compared to distal areas is well documented. However, there are no data of sedimentation rates in other littoral zones not affected by spring discharge, although it is said that rates higher in spring than in non-spring shallow areas. I think this part of the discussion could be expanded including some data from the Vogel et al. 2010 and Matzinger et al. 2006 cores.

+ We agree and refer to our response upon the identical questions by Reviewer 1 stated above: We appreciate these comments and agree that a comparison with other cores would be beneficial. We added thus more comparisons to the deeper-water cores from Matzinger et al., 2006b (explained above), however, no comparable littoral cores are available. There are cores from a subaquatic terrasse at water depths of 30 m and deeper (Lindhorst et al, same issue), but they offer no comparison, as they are in a much lower-energy environment and furthermore have totally different lithologies as Late Glacial deposits occur already in few cm depth.

The design of our coring transect somehow allows such a comparison, as the northern transect moves more than 1 km away from the spring area. This provides some clues, how the local spring hydrology does affect the sedimentation pattern. We do not have other data at hand, that would provide shallow water sedimentation from farther away but we believe that at a km-distance, the effect of the spring should be minimal as the waters are strongly diluted.

The authors claim that the depositional change between unit L and T after 1955 is mostly associated to increased eutrophication of the lake. Although I agree human impact in the watershed is likely the culprit, more discussion would be needed to support this hypothesis. Increase in TOC and diatoms suggest higher organic productivity (although the caveat with diatom preservation has to be considered too). Why Fe increases in the upper unit?, soil erosion?, What is the contribution of these processes to littoral sedimentation?, do they have an impact on spring evolution?. Is the decrease in TIC a measurement of increased eutrophication in the lake?. Actually, in some of the distal cores (Matzinger et al., 2007), a slightly increase in TIC occur at the top of the cores (with TOC, and TP). Should we expect stronger indications of eutrophication in the littoral zone (closer to the source) and weaker closer to the springs (higher input of non-polluted waters) ?, Are similar trends described in other cores in non-spring areas?.

+ As stated there are no other cores from shallow-water non-spring cores that could be used to elaborate on this issues. But we refer to our comments in context of the eutrophication in section B above. We conclude that it is a lake-wide phenomena, which becomes enhanced and more sensitively registered in the shallow-water areas.

Besides the increased in nutrient to the lake since 1955, the hydrological change caused by the diversion of the river Sateska may be quite significant in terms of suspended sediment load. Could that be a reason for the Fe increase in the upper unit?. Although unlikely, this possibility could be explored since it would have a clear impact in sedimentation rates. Changes in lake chemistry (dilution, lower alkalinity) caused by the increased river inflow could also account for better diatom preservation

+ This is a good point and we added that this deviation also provided more sediments (and not just nutrients) to the area. We already state at the end of section 5.3 that Matzinger et al (2007) considered the river to be not responsible for the nutrient

contribution. Whether it is responsible for changes in geochemistry and sedimentation rate is hard to evaluate and we prefer not to single out one out of several possible interpretations for the observed changes, as we do not have the 'smoking gun' at hand.

Figures.

It is somehow confusing that different cores are used in Fig. 6 and 7 to show sedimentological properties and geochemical properties. I would add a small map with the cores location in those figures.

Good suggestion: We added a small insert in both Figs. 6 and 7 showing the locations of the displayed cores.

Reviewer 3

General comments:

The study is a contribution to the understanding of shallow-water, groundwater-mediated carbonate precipitation (with the complication of and sediment transport in Lake Ohrid, a future scientific drilling target and an oligotrophic lake with recent anthropogenic nutrient inputs). The methods are well presented and justified, although some could be better supported by references. Some of the conclusions (including the anthropogenic nutrient loading source) are equivocal due to problems with sedimentary chronology and the dynamic nature of the shallow water environment, which make it difficult to reach conclusions about paleoenvironment and changes over time.

Specific comments:

Throughout the paper the authors casually shift between use of "TIC," "carbonate," and "calcite" when referring to analytical results and sediment composition. TIC is carbon in carbonate, carbonate is a class of minerals or the carbonate ion, and calcite is the specific carbonate mineral, and they are not interchangeable. This inconsistency is confusing, but more importantly in some cases (especially when referring to percentages) is incorrect. For example, 4716 / 13, "carbonate contents of up to 96%" is clearly referring to carbonate mineral content, as it is impossible to have 96% TIC; whereas 4724 / 4-5, ". . . measured carbonate content (TIC)" mixes mineral or anion and carbon fraction. Also, the authors assert that the carbonate mineral fraction was confirmed to be all calcite, but it is not possible to determine mineralogy conclusively by either of the methods listed (petrography or SEM/EDX); petrographic smear slides are an excellent method, since carbonate crystal form is indicative of mineralogy, but it is not certain. If carbonate is indeed all calcite (as verified by x-ray diffraction, for instance), say "calcite" instead of "carbonate" for clarity. With respect to the authigenic calcite crystals, "idiomorphic" is a less commonly used, and less descriptive, word to describe these grains than "euhedral" or "rhombic." To be extremely picky, "idiomorphic" in fact means that the crystal growth has not been interfered with, while the paper gives examples of cyanobacterial or picoplanktic mediation and disruption of the crystal form. The paper lacks references for the interpretation of calcite shape significance; what physical and (bio)geochemical processes are implicated in formation of the very large calcite crystals (or aggregates) putatively transported from shallower water?

+ This is a good point, and we made the manuscript more consistent by referring only to carbonate % instead of % TIC in the results and discussion sections (assuming all TIC is bound in CaCO_3). We left TIC in the method section, as this is the parameters that is actually measured and mention how it refers to carbonate %. We also only use carbonate % in Figs. 7 and 9 (x-axis in Fig. 9 was changed from TIC to carbonate %). In the Table, we prefer to show TIC values, as this was measured.

We are confident however, that we mostly deal with calcite crystals, as both aragonite and dolomite are not expected to be formed under these shallow-water conditions, though no XRD analysis were performed.

magnetic susceptibility values are missing factor of 10^{-6} . This is a common error in reporting MS from Geotek instruments.

+ This is correct and we indeed provided the wrong dimensions of our measurements. As they were measured in SI units, the proper exponent is 10^5 and we corrected this in the manuscript.

The issue of reworking and sediment transport in these littoral environments may be more important than acknowledged in the paper, given (1) the problematic radiocarbon, lead-210, and cesium-137 dates, and (2) the difficulty in correlating cores along the transects, which could be due to the removal of entire sedimentary units. In addition to longshore transport, wave action may be important: what is the depth of wave base in Lake Ohrid, with respect to the depths at which cores were taken? These are not of course problems particular to this study, but to any shallow-water depositional environment, and I laud the authors for taking many cores and attempting to relate their composition to both local and littoral-wide processes.

+ We agree. We are discussing this things more intensely in the revised 'age model' section of the manuscript (see also comments on age-model changes in A above).

The sediment traps in the study were deployed for only a short time; it would be more convincing to see longer time periods, especially when reworking is a major process.

+ We fully agree with this statement, but unfortunately, for logistic reason, no longer measurement period was possible. Nevertheless, we believe that the two-week period provides crucial and meaningful data, which we present with the necessary caution.

4731 / 4-5, decrease in %TIC is probably due to dilution by other components (TOC, diatoms, Fe-bearing minerals, etc.) that are seen to increase over the same period; that is, overall sediment accumulation rate may have increased under anthropogenic forcing. Dilution is mentioned in the last paragraph of the conclusion, but not earlier in the discussion of anthropogenic effects.

+ The process of dilution (in respect to diatoms) is now mentioned more upfront and also incorporated in the discussion of the interpreted eutrophication.

While the absence of corrosion features on the calcite surfaces is good evidence that waters were (almost) always above calcite saturation, but I would ideally like to see more seasonal water chemistry data (only May and September are shown) as well as more data and saturation indices for bottom waters, where conditions may be more corrosive.

+ As mentioned above, water chemistry data is not available at seasonal resolution.