

## Response to Referees

### ***The last glacial-interglacial cycle in Lake Ohrid (Macedonia/Albania): testing diatom response to climate***

***J.M. Reed, A. Cvetkoska, Z. Levkov, H. Vogel and B. Wagner***

The authors would like to thank the three named referees (acknowledged in the draft manuscript) for providing such detailed comments on the manuscript. To prepare a response, each comment is dealt with in turn; they were many and varied and we have selected those upon which to focus to avoid losing the central focus of this low resolution analysis on exploration of diatom response. As argued below, some other suggestions would be more appropriately addressed in a future, higher resolution study. Comments regarding text have also been addressed by editing the draft manuscript in Word using Track Changes (attached); this cannot easily be attached.

We have included at the end of the document new draft versions of Figures 2-4, a new Figure which displays the age-depth model and sediment accumulation rate. Large diatom image files of *fottii* and *ocellata* have been omitted but are prepared. Figures 3 and 4 have some suggested additions already and we intend to add tephra, MIS stages, IRD data to Fig 4.

In response to suggestions to explore the data in more detail, scatter plots are included which compare CA with PCA scores for the UK merged data-set, and which compare variability in dissolution classes between UK and Macedonian data-sets. It is not intended to publish the additional exploratory scatter plots.

#### **RESPONSE 1: HOLGER CREMER**

##### **PAGE 2/4690: "better to use diatom accumulation"**

Done

##### **PAGE 5: "ultra-oligotrophic? Please provide P, Si and Chl a concentrations, if available. I suggest including a table summarizing modern limnological conditions"**

Using the OECD boundary of 2.5 µg/l TP, Ohrid varies between oligotrophic and ultra-oligotrophic, and did so even in the 1930's. I have amended the text to explain this in more detail than previously, including brief statistics from the 1930's, Stankovic (1960) and Matzinger's recent water chemistry monitoring to summarise the character of P, nitrates and silica. I could not find chl $a$  data. We would prefer not to include tabulated data because it would lengthen the manuscript too much and is not so relevant to the paper.

##### **PAGE 6/4694. "was sediment freeze-dried before?"**

No.

##### **PAGE 6. "relatively good is a relative qualifier. Can you quantify this, define a clear index? Perhaps it would help to add a plate showing examples of *C. fottii* and *C. ocellata* in good, moderate and bad conservation".**

I agree 'relatively good' is a loose term. It has been replaced with a truly unquantified term, 'sufficient'. It was not quantified – where diatoms are so poorly preserved that counts of ca. four

transects only produce a total of less than ca. 50 valves, I generally deem it a slide which on which a full count is not feasible, and one where counting 500 valves would not actually improve the result because highly dissolved assemblages are invariably dominated by very few taxa. The text has been edited to clarify this.

To clarify the manner in which dissolution of *C. fottii* and *C. ocellata* occurs we have prepared two plates in draft form, which are attached. The Methods section now has the details of the camera added (and the Macedonian microscope). Relevant to comments below (and other referees' comments), this also displays the two different forms of *C. fottii* (central area robust and dissolving last, vs. central area with large depressions which result in dissolution of the central area prior to dissolution of the marginal striae), and a selection of the *C. ocellata* morphotypes.

**PAGE 7/4695. the taxonomical problems of *C. ocellata* are still not entirely solved, in fact we don't know how many species are included in the ocellata complex. It probably includes more species (*C. rossii*, *C. krammeri* et al.) and using ocellata complex is therefore better than ocellata. You should point out the tax./morphol. problems in more detail, perhaps under a header Diatom taxonomy. Perhaps you may also consider *Algological Studies* 116: 147-169, 2005 and *Algological Studies* 86: 17-37, 1997. Both papers deal with the morphological variability of *C. ocellata*.**

We have added Hegewald & Hindáková *Alg Studies* 1997; Cremer et al. 2005 (*Alg Studies* 2005), plus other references to the manuscript, making it clearer that *C. ocellata* is understood to be a complex. As noted above, we can provide a plate displaying the main elements of morphological variation within the *C. ocellata* complex. A full taxonomic paper is planned for future publication, and we hope you will agree that it would be more appropriate to go into greater detail in this paper, including publication of many plates. We feel it would detract from the primary palaeolimnological focus of the paper to add too much detail here, particularly because the more detailed taxonomic data-set is not used for the palaeo analysis.

**PAGE 8/4696 - "how were valves of *C. ocellata* with 1 , 2 or no ocelli treated? I assume they certainly exist. Again, and because the size class approach is crucial for this paper, I suggest to include a plate showing examples of the size and morpho-classes for both *C. fottii* and *C. ocellata*."**

It may be unusual, but neither laboratory identified ANY valves with 1, 2 or no ocelli, apart from in the smallest size category where ocelli could sometimes not be distinguished at all. Size classes were only used in the main analysis, to split *C. fottii* into two size classes. They were not used for *C. ocellata*, apart from the smallest size category. The morpho-classes are displayed in the proposed plates. Again, we hope it is acceptable to probe the taxonomy in more detail in the separate paper, because it would confuse the reader to add taxonomic detail from the harmonization study which was not then employed in the main data analysis.

**PAGE 8 - "include a table, possibly as online supplement, with the complete taxa lists handled by both groups. Were the defined size classes considered as taxa?"**

As noted above, we would prefer to include this in the taxonomic paper to avoid duplication in the future publication; palaeolimnology papers do not often include such appendices. The adoption of size classes was outlined above; we hope the existing text is clear in explaining that, while the Macedonian group split extensively into different size classes, it was the UK data-set which was used subsequently for palaeolimnological analysis. In the CA, the two *fottii* size classes, being kept separate, were analysed as separate taxa, as were valves of *C. ocellata* with different numbers of ocelli. Since (as discussed above) it is not yet clear where the species level lies in either taxon, or the degree to which morphological variability at a sub-species level may be influenced directly or

indirectly by climatic parameters, we hope you agree that it is appropriate for the current study to decide on the appropriate degree of splitting, as we have, by comparison of degrees of variability using ordination techniques.

**Page 8 - "I would not consider the differences as being only little ... at least it indicates that there should be only one counting method in future studies."**

We have edited the text to quote the differences in percentage variance. The ordination techniques are explored further in response to referee 3 and the new results are presented in a new Figure 2; please see below.

**PAGE 11/4699 - "indicate the tephra layers in Figure 4."**

We have the data on depths (and therefore ages) of tephra layers and can edit Figure 4 to display them; depths are as follows:

The nomenclature and depths for the tephra and cryptotephra layers according to Vogel et al. 2010 JQS: OT0702-1/74.5-77.5 cm (cryptotephra), OT0702-2/145.5-144cm (cryptotephra), OT0702-3/269-277.5cm (cryptotephra), OT0702-4/617-620cm (tephra), OT0702-5/689-696cm (cryptotephra), OT0702-6/743-752cm (tephra), OT0702-7/822-825cm (cryptotephra), OT0702-8/1140-1146.5cm (tephra), OT0702-9/1229-1232.5cm (tephra), OT0702-10/1440-1447cm (cryptotephra).

**PAGE 11 - C. fottii is considered in the paleoreconstruction a cold indicator, C. ocellata generally a warm indicator. I.e., high rel. abund. of both species point to higher/lower paleotemperatures at the site. Temperature certainly has an (direct or indirect?) effect on the presence/absence of diatom species in various environments but many other factors, including lake-inherent factors, certainly play also a role. C. fottii is the dominant diatom in the entire record, it obviously grows during both warm and cold periods. This observation alone indicates that temperature is not the only (perhaps not even the main) trigger. C. ocellata is a cosmopolitan species also found in cold environments (Algological Studies 116: 147-169, 2005; Diat. Res. 23: 75-91, 2008). Also, other long records of C. ocellata (for ex. JOPL 37: 155-162, 2007) should be considered; the mentioned record shows that C. ocellata dominates during both warm and cold periods, indicating that changes in rel. abund. must have been triggered (at least directly) by other factors than temperature. Perhaps the authors can add a few lines to this in the discussion.**

We agree. The diatom response is not predictable, and tends to vary between lakes depending on factors such as depth and productivity. Studies in Ohrid are in their infancy; the importance of this study is to demonstrate the high sensitivity of diatoms to relatively well known episodes of climate change but we can as yet do little other than speculate as to the manner in which climatic forcing functions drive limnological change. I have edited the text (as also suggested by referees 2 and 3, below) to strengthen the discussion of interpretation and associated uncertainty and to extend discussion of non-planktonic taxa. Interestingly, from a study of Lake Ioannina, NW Greece, by the first author and others (in prep.), interpretation can also be complicated where the response can change over time in a single lake basin as a consequence of long-term variation in limnological response thresholds with lake ontogeny. Further research is necessary.

**PAGE 13/4701 - "are oxygen isotope measurements of diatom silica planned for future activities?"**

I understand oxygen isotope analysis of diatom silica suffers from difficulties of contamination. My guess is that diatoms are not sufficiently well preserved in glacial periods to produce a reliable record and we have no plans to pursue this ourselves.

**PAGE 14. - "diatom accumulation?"**

OK

Page 15/4703. - "easy to do by using Battarbee trays; better to calculate diatom accumulation."

Neither lab has tried this. I am aware that Rick Battarbee's own lab prefers to use microspheres so have always assumed this is the most reliable technique, even if it is time consuming.

**PAGE 15 - "A marine study (Geologische Rundschau 84: 108-136, 1995) nicely shows that living communities, sinking and sediment assemblages are (largely) affected by selective dissolution."**

Thanks for suggesting this paper. I think the cited Baikal studies (e.g. Flower) also show this, and the paper is now very long, so I hope I may leave it out.

**PAGE 16. - "why not? I could imagine that lake water chemistry was affected in a way causing such a permanent shift".**

The geochemistry does not indicate this so I would remain cautious. If chemistry were affected, you would expect different species, rather than just morphological variability? I have added a comment to this effect

**Page 16 - "In future, the taxonomy *C. ocellata* needs to be studied in detail, possibly also including the genetic variability. The work in *Algol. Studies* 86: 17-37, 1997, clearly shows that all morphological variants occur independently of temperature and chemical changes. It's absolutely not clear how such morphological changes are triggered."**

We agree; analysis of a larger number of modern/palaeo lake sequences might allow some of the potential mechanisms to be tested, but we cannot do so in a single study. I have extended the discussion of morphological variability to include this and other relevant references.

**Page 26 - "please include *C. fottii* (total) into this figure".**

This has been added to Figure 4 (new version attached).

## **RESPONSE 2: ANSON MACKAY**

**More detail could be given on proposed mechanisms between climate change and diatom assemblage composition, e.g. through shorter/longer duration of ice cover, summer stratification, nutrient availability etc.**

We hope you agree the discussion has been extended accordingly. We have tried not to make the paper too long, since most of the interpretation is currently speculative. We aim to test some of the ideas in future work.

**P4691, c. line 11: pollen is also consistently preserved in LB sediments, but have been much less extensively studied**

I meant for limnological reconstruction – have edited to clarify.

**P4692: in the examples being given here, might be worth referring also to Rioual et al. QSR (2007) and their detailed diatom work for the period 130— 105 ka BP and the Last Interglacial in particular at Ribain's Lake, France.**

Thanks very much for reminding us of this paper. We have included it in the Introduction as a key study, mentioning that it is a more high altitude lake.

### **Methods**

**I think that it would be useful to include the age model figure from Vogel et al. 2010a (JQS)**

We agree. A diagram is attached, which can be added to the manuscript without copyright problems.

**State how many samples were actually analysed for diatoms**

63 – text has been edited.

**What was the rationale between two different laboratories counting diatoms on the same samples?**

It was for taxonomic harmonization and to set protocol for taxonomy and dissolution analysis. We are not surprised by the comment by Referee 2 (and 3) that it seems unusual to count a complete sequence twice. There were various reasons for this, both practical and academic. The approach (as noted in the text) gave the advantage that it allowed a test of the variability in ordination results for different splitting procedures, which is important given the increasing tendency to compare Axis scores with global climate change parameters in palaeolimnological research, and also allowed dissolution to be expressed in more detail. This could not have been done in the same way by progressive merging of the Macedonian counts since a greater proportion of valves are classed as dissolved at the higher level of taxonomic resolution. Although we agree this might be a lot of extra effort, there were also practical reasons. We carried out the study remotely without being able to meet up initially for a taxonomic workshop. For reasons of workload, submission deadlines etc, the two laboratories could not count the sequence at the same time. The Macedonian team (masters student plus supervisor) had completed their counts before the UK side began. This meant that more detailed dissolution criteria could not have been adopted if these were the only data published. Although the first author could simply have written a paper using the Macedonian data, it may have lacked scientific rigour; the necessary depth of understanding of the data comes only from counting, so the benefits of double counting outweighed the effort involved.

**A figure showing main morphotypes for each *Cyclotella* species would be a useful addition here**

We agree (see response to Ref 1, above) and have prepared draft plates both of *C. ocellata* and *C. fottii*.

**Why was CONISS used as a zonation technique and not arguably better techniques such as optimal partitioning (Birks & Gordon 1985)?**

The honest answer is habit – I had not explored using optimal partitioning. Having done so now, it appears that optimal partitioning works better than CONISS when there are zones of transition, and that more zones would be found in a long sequence such as this depending on the resolution of counts. Since this is an exploratory paper with a low resolution count (and therefore one in which most transitions appear to be abrupt), I have not changed the analysis. I would certainly explore its application in a higher resolution analysis.

**With regard to the ordination techniques, a little more detail/justification could be given for different options chosen. For example, given such a low gradient (<2.5 SD units), PCA might be a more appropriate technique, given ‘arch’ problems encountered using CA.**

**Also, why were rare species down-weighted?**

Referee 3 also discusses the ordination techniques. For clarity’s sake, all ordination comments are dealt with below in Response 3.

**Results**

**P4697, line 8: replace ‘CA results...’ with ‘Diatom results...’**

Done.

**How do the different datasets compare if the Macedonian categories are combined. For example, if the 8 size classes for *C. fotti* are combined to >20 and <20 um (similar to the UK dataset?)**

Graph is attached which compares dissolution classes (but is not intended for the manuscript). As noted above they are not absolutely equivalent because different proportions were assigned to ‘dissolved’ in the two data-sets.

**If the *Aulacoseira* sp. is unidentified, are the authors certain it is endemic? What other information do we have about this taxon?**

We have had a chance to explore this further since first submission. From SEM study, the taxon is very similar to *Melosira temperei* Pantocek (Köpecz Miocene type site, Romania, and first described in Macedonia from Pontian deposits of the eastern Galicica Mountains, by Nadja G Ognjanova-Rumenova in:

Dumurdzanov, N., 1997, Lacustrine Neogene and Pleistocene in Macedonia: Proceedings of the Field Meeting held in Yugoslavia in 1995–1996, International Geological Correlation Program project 329, “Neogene of the Paratethys”: Belgrade, Special Publication, Geoinstitute, no. 21, p. 31–36.

See also Václav. H. Klee R. 2007. Atlas of freshwater centric diatoms with a brief key and descriptions. Part II. Melosiraceae and Aulacoseiraceae (Supplement to Part I). Fottea 7(2), 85-255. Table XCVI:

Shared morphological characteristics include spatulate linking spines, the same location of stalkless rimoportulae on the internal part of the ringleist, and the same form of the rota (internal areolae).

This detail belongs in a taxonomy paper rather than here; we have included a note that it is probably *Melosira temperei* (with a reference) but have left it as *Aulacoseira* sp., with an additional comment that the taxon is probably relict – it is not necessarily endemic but is characteristic of Miocene deposits of the Balkans.

**P4697, lines 19+: photographs here would be useful to confirm statements linked to morphological variability**

Included on the plates described above.

**According to the CONISS output on Fig 3, the most important boundary between zones is between COD2b and COD3**

Yes- text for COD1-COD2 is edited accordingly.

**The results section is a mixture of purely results, but also some interpretation as well – perhaps leave all interpretation to the discussion to avoid potential repetition? (e.g. p4698, lines 1-2)**

OK – relevant sentences are removed, and in one case (final para of Discussion) the text has been extended slightly to cater for this.

**In fig 3, given that the chronology is well established, it would be useful to either plot alongside the depths, approximate ages, or at least give ages alongside the zone boundaries**

Agreed – I have added an age scale y axis to figure 3 and have edited the text to include ages at boundaries.

Discussion

**In fig 4, I would add in total *C. fotti* species too, as this taxon has clearly been indicated to represent cooler prevailing climate, in contrast to *C. ocellata* – or is this taxon already included in FP+benthic? More clarity here needed**

Done

**Relating tephra layers (in cm) to age-scaled Fig 4 is not particularly useful. These observations should come in the results section**

We suggest following Ref 1's suggestion that tephra is displayed instead on Figure 4 (citing both age and depth in the m/s), as it links more to interpretation than results.

**P4700, line 25+: which aspect of the geochemical record suggests a temp minimum at 108.4 ka? It is not apparent on fig 4. The corresponding 'peak' in *C. ocellata* looks to be part of a fluctuating curve, and given the resolution of the diatom analysis and uncertainty in the dating, I'm not convinced that the statement ". . .suggesting correlation with the North Atlantic C24 event. . .is unreliable." is warranted.**

The evidence was 'stepwise decreasing calcite contents from 110.2 ka to a minimum at 108.4 ka indicate more pronounced cooling'. This is indicated on Figure 4; it is rather tentative on its own, as acknowledged, and is followed by a hiatus in the core. Given the otherwise close correlation between *C. ocellata* and geochemical variation (until the Holocene boundary), the peak of 22% *C. ocellata* at 108.3 ka does urge caution. We acknowledge the uncertainty due to the currently low resolution of diatom analysis and have edited the manuscript to make the statement less forceful.

**While P4701, lines 3-7: this section seems a little confused. With regard to the Baikal comparison, diatom evidence for cooling starts at c. 119 ka BP, and minimum productivity is reached by c. 117 ka BP (Rioual & Mackay 2005; Mackay 2007). Biogenic silica evidence suggests that minimum**

**productivity is reached by c. 115 ka BP (Prokopenko et al. 2006) with the start of MIS5d and full glacial conditions. Finally, the reference Mackay (2007) is misplaced, as it suggests that that study was related to Ohrid.**

We have included additional references and added detail to this section, also adding brief mention of Lago Grande di Mont. as a more local site with which Vogel et al. have already compared the sequence. We do not want to go into too much detail on Baikal (or other long sequences) here because we feel it would detract from the internal focus on Ohrid, which was the main aim of this exploratory paper. A full 'global correlation' would be more appropriate for a higher resolution study. I have edited the placement of Mackay (2007).

**The diatom data for MIS 3 are indeed very interesting. Clearly, the zonation demarked between COD3 and COD4 highlights the potential correspondence between diatoms and the start of MIS3. Throughout the whole of the MIS3 (i.e. zone COD4), the most characteristic feature is the presence of *S. pinnata* – could the authors expand more on its significance during this period? For example, could it be that ice cover duration declined enough to allow a littoral community to establish in the lake?**

Having followed the suggestions by Refs to extend the discussion elsewhere, an extra comment has been added here (linked to the COD1/2 interpretation) regarding the interpretation of the small Fragilariales here. Thanks. The main discussion of small Fragilariales has been given within COD1/2. We agree that reduced ice cover is the most likely explanation (citing Smol and Douglas 2007 in support), but argue that in the case of COD1 in particular it is difficult to explain the variability simply in these terms.

**Finally, given that the authors have compared their study to Baikal for MIS5e/5d, they could also compare their data for MIS 3 in Baikal (Swann et al. 2005). For example, Swann et al. also demonstrate diatom evidence for warm conditions between c. 39-35 ka BP. However in Baikal peak warming was probably during the start of MIS 3 between c. 54-51 ka BP, so here the Ohrid and Baikal records appear to differ substantially.**

The above response concerning comparison with Baikal data applies. The paper will become diffuse if we extend it too much, and would prefer to save more detailed comparisons for a higher resolution study. We have noted instead that the ca. 59 ka start of MIS3 in Ohrid corresponds well with the MIS boundary.

**P4702, lines 16-17: the authors could be more specific for dates for the start of late glacial warming (c. 14.7 ka BP) and the start of the Holocene (c. 11.7 ka BP), given the resolution they ascribed to earlier transitions during e.g. MIS 5e and MIS 3.**

This was deemed unnecessary because evidence for late glacial warming and an abrupt transition to the Holocene are lacking in this sequence, but we have edited the text to be more precise.

**P4702, lines 26-27: does the sediment record show evidence for IRD during the early Holocene as well?**

The main IRD evidence was qualitative, comprising observation of gravel grains at 1494-1439 cm and 1064-246 cm. This extends into the early Holocene, but the question of mixing vs. a prolonged cold phase remains.

**P4702, lines 27+: could it be that the diatoms are not as sensitive to changes in climate as the authors suggest, rather than the other way round? The resolution of the profile overall is rather low, and does not easily allow transition periods or abrupt changes to be interpreted in general (which the authors themselves acknowledge on page 4703).**



We doubt it – the entirety of zone COD5b (into the mid-Holocene) lacks evidence for warming – this is not just an abrupt event. As argued in the text, we regard the presence of Cyclotella-dominated assemblages in the south of the lake during some of this phase as evidence (in this fairly small and simple graben) that diatoms are sensitive, and it is also supported by the evidence for warming in MIS3. Thus we argue that the most likely explanation is that the Co1202 sequence is compromised at the start of the Holocene. An alternative explanation – that this part of the lake basin did indeed retain its glacial character for a more prolonged period - is raised by Referee 3. As noted below a long-core transect study would be necessary to test this, but it seems unlikely due to the relatively small size and simple morphology of the lake basin.

**P4704, lines 10-12: ought it be implicit that size classes are a feature of evolutionary pressures?**

We were not assuming size should evolve, just demonstrating that it didn't appear to have done. It is not beyond the bounds of reason; Schlegel and Scheffler 1999, for example, have demonstrated a link between valve diameter and shifts in water chemistry of *C. ocellata* during the annual cycle, so long-term environmental change might have this effect. I've edited the text to make this a little more clear (and have extended this section following Ref 1).

**RESPONSE 3: DAVID RYVES**

**General comments**

**This is a well-written, thoughtful and interesting paper that contributes valuable new data on a key site for late Quaternary climate change from this important region. Using diatom morphological and dissolution data provides an extra dimension to investigating environmental change and taphonomic processes, and certainly this paper will be important baseline study for later higher-resolution work from this site. The paper confirms that the diatom record is a sensitive proxy of climate/environmental change, but not without its own challenges and complexities that will richly reward further study. I have a few, more substantial comments, given below, and a list of typos/minor points.**

**No response necessary**

**Specific comments**

**1) Methodology**

**a) 4694 L12-16 Why different preparation methods used in UK and Macedonia? effects on diatom representation e.g. smaller (dissolved/weakly silicified) taxa – though preservation not affected (how was this tested?).**

The effects of different preparation techniques were only explored qualitatively, by the Macedonian masters student. Slides were screened to assess preservation visually, and there appeared to be no discernible difference. As noted above, we were working remotely and at different times, so Reed adopted her standard technique after correspondence with Macedonia. In the event, the Macedonian collaborators have later confirmed that the final preparation omitted the KMnO4 stage, so the two laboratories' techniques were almost identical. Text has been edited to remove this stage from the Macedonian methodology.

**Might be interesting to see a scatterplot of dissolution indices of UK vs Macedonia, to check dissolution protocol consistency (though differences might be due to preparation method too).**

A diagram is attached for information, comparing dissolution classes. As noted in response to Referee 2, the two methods produce different results.

**b) Why was parallel counting employed – on same samples? If a smaller calibration set counted in common (to establish taxonomic/preparation consistency), then twice as many samples could be analysed between the 2 teams?**

See Response 2.

**Also, some lumping of the Macedonian data to approximate that done in the UK (merged or split) would likely show the same CA patterns (cf. Fig. 2).**

See below: discussion of ordination and new Figure 2.

**c) Diatom samples from Baikal sediments, which are also poorly preserved generally, were prepped using water and overnight settling alone (see Mackay references) – seeing the low TOC% at Ohrid, H<sub>2</sub>O<sub>2</sub> might not be necessary and in future work, this might minimize further valve destruction/loss etc.**

Thanks very much. We will investigate this.

d) Perhaps use Battarbee et al., 2001 rather than Battarbee, 1986?

I still use the technique picked up from Battarbee, 1986, so prefer to cite it.

**2) Fragilariales are notoriously difficult to interpret – but I think the authors are right that diatom concentration data, and flux data, would be very helpful in determining to what extent planktonic and benthic accumulation rates differ between glacial/interglacial cycles, and their relationship to dissolution indices. Such data would also help interpretations of changes in lake functioning and limnological processes (e.g. ice cover, stratification, water residence time, nutrient enrichment, lake level change etc) that might be the factors to which diatom communities are in fact responding (and this might in turn shed light on the different time lags expected for different limnological factors – e.g. rapid response of ice cover for example compared to generally longer changes in nutrient levels mediated via catchment changes in weathering, soils and vegetation, and internal cycling).**

See above Responses to Referee 2, and extended discussion of ‘warm phases’. We hope a higher resolution study, couple with future modern limnological study (and the results of ongoing pollen analysis), may in time allow us to disentangle some of these variables. Equally, when the long core is drilled, probably in a different location, we hope to be able to test the relative influence of river inflow etc. across the lake.

**3) Dissolution: The pristine category used here equates to the F index of Mackay and others, and does show good correspondence between glacial (generally poor) and interglacial/interstadial (generally good) conditions. It is possible an index involving more dissolved categories (e.g. Ryves et al., 2006) might provide a better summary, as there are 3 categories for *C. fottii* and *C. ocellata*.**

We actually need to develop an index which takes into account the strange and shifting pattern of dissolution of the two morphotypes of *C. fottii*. It was not feasible to count the sequence a third time, so improvements will need to be attempted in a future, higher resolution, study. Thanks for suggesting the paper – we hope we can use the results of your studies to devise a more effective system for representing dissolution.

But I also noted that in Fig. 4, the correspondence between pristine valves and CA axis 1 scores seems strong in a constrained ordination (time or depth as co-variable; and perhaps with MIS or cold/warm as co-variable too?...), I wonder how much variation in the diatom data (lumped) would be explained by dissolution? Of course, one can argue that in an assemblage where a handful of taxa dominate, CA axis cores reflect the dominant taxa, which also dominate the F index scores – but perhaps interesting nonetheless to explore!

**Combined response to Referees 2 and 3 on ordination:** In the submitted Figure 4 the correlation between pristine valves and CA Axis 1 scores was indeed extremely close. I had thought this reasonable because it is such a low diversity sequence. However, in investigating ordination techniques in more depth, have to admit that the reason was simply because, in haste, Tilia sums remained in the data-set used for initial CA analysis of the UK data. CA has been run again for the two UK data-sets and the results section is edited accordingly, providing also data on DCA gradient lengths, percentage variance contained in axes 1 and 2 of each CA analysis. A new version of Figure 2 (attached) shows close correspondence between the pattern of variability in the two UK data-sets and the Macedonian data-set. Analysis of the Macedonian data-set still exaggerates the importance of *C. ocellata*, however, so it is logical to use the UK split data-set in the subsequent palaeolimnological analysis, as was done originally. Referee 2 asked why rare species were downweighted; running CA without downweighting showed the undue influence of a single very poorly preserved sample which comprised 100% dissolved *C. fottii*, confirming Ref 3's suggestion that variability in dissolution may explain a significant proportion of variability. The single sample was removed in final analysis, as now explained in the manuscript, and we have presented results without downweighting rare species. Figure 4 has been edited to display the new CA Axis 1 scores.

**4696 L 14. Linear ordination might be appropriate for gradients <1.5-2; what were the gradients for diatoms?**

This is a grey area; Referee 2 suggests PCA for gradients <2.5. The UK merged data-set was the only data-set with a gradient <2. A figure is attached comparing PCA with CA for this data-set, which shows that PCA and CA patterns of variability are extremely similar. For consistency, we prefer to keep all analyses as CA. Gradient lengths are now reported in the manuscript.

**4) Figures illustrating the main taxa (or just *C. fottii* and *C. ocellata*), and their dissolution morphologies, would also help and be of interest – the pattern of *C. fottii* corrosion alluded to is certainly unusual for *Cyclotella* as observed elsewhere (e.g. ...Battarbee, 1988; Flower, 1993; Ryves et al., 2009).**

Done – see above.

**5a) Differences between present and last interglacial – clearly the appearance of *S. galileensis*, and importance of *C. ocellata* (3 ocelli) plus benthic differences are the major contrasts between these two interglacials – more discussion on what these differences are, what they might mean and wider implications would be useful.**

Again, more extended discussion is perhaps appropriate for a higher resolution paper. We put forward enhanced nutrient availability as an explanation of the *Stephanodiscus* – results of pollen analysis might allow us to test whether this is due to differences in catchment processes/vegetation development, or to lake ontogeny (?long-term shallowing) for example. Ref 1 discusses the

uncertainty of interpreting morphological change, and comments have been added to this effect in the morphological variability section.

**b) The discrepancies in location of cores Co1202 and more littoral Lz1120 (plus location closer to major inflow etc?) may explain differences between these records, but potentially so could dissolution (better preservation in Lz1120?). But if there is evidence of ice-rafting in Co1202 (when/how much? Curve of this in Fig. 4 would help), this would suggest that conditions here were colder, perhaps into the Holocene, and thus Co1202 might not be as compromised as the authors suggest – but then one would expect to see similar cold conditions at Lz1120 too, only 20km away. How sound is the dating for Co1202 at this time (or indeed Lz1120)? Both cores are on slopes – could there be effects from turbidites or other sedimentological processes affecting the integrity of the records?**

The dating for both sequences appears sound, with Holocene radiocarbon dates and tephra layers producing consistent chronologies. Wagner/Vogel carried out a detailed seismic survey prior to selecting coring sites. The two sites are both well selected (unlike some other recent studies which have been attempted). The hiatuses in the sequences might indeed be due to slumping, but to the best of our knowledge the chronology otherwise is reliable. Pollen analysis is not yet complete on Co1202; this may serve to answer some of these questions. If the chronology is accepted, the good preservation of *C. ocellata* in the Last Interglacial and later Holocene sequence in Co1202 argues against heightened dissolution in this site compared to LZ-1120 being the main explanation for the early Holocene lack of warming evidence, and we agree that this basin is too small to expect major differences in ice rafting between two coring locations. In short, as noted above, we feel strongly that the presence of earlier Holocene, well preserved assemblages in LZ-1120 does suggest compromise in Co1202. The IRD data are qualitative gravel-sized grains; as noted above, the levels observed could be added to Figure 4 if requested.

#### Figures

**Figure 3. This would be easier to read & interpret if there was a 2nd Y axis with the dates (I see that plotting against age might bunch up the counts too much);**

We have added a second y axis with dates in the new Figure 3.

**and another column with the MIS stages would certainly help.**

This might be better displayed on Figure 4, which links to the interpretation. The MIS stages would be displayed as follows:

#### MIS boundaries

1 to 2	11.7	more accurate than 12 (Anson); start of LG warming 14.7 (Anson)
2 to 3	24	
3 to 4	59	
4 to 5a	71	
5e to 5d	119 +/-2	
5 to 6	128	sensu stricto 130 +/-2

REF: Imbrie, J., Hays, J.D., Martinson, D.G., McIntyre, A., Mix, A.C. et al. 1984. The orbital theory of Pleistocene climate: support from a revised chronology of the marine d18O record. In Berger, A.L.,

Imbrie, J., Hays, J.D., Kukla, G. and Saltzman, B. (eds), Milankovitch and Climate (Pt. I): Dordrecht, Reidel, pp. 269-305.

**Also, values below about 2% are very hard to discern on this plot – could these low abundances be indicated by a + symbol etc? Another way to do this is to remove the baseline (y axis) for species plots, thus all occurrences can be seen.**

We hope instead that the editors can produce a larger diagram on the page, because the diatom diagram in the draft manuscript is very small.

**A plot of the age-depth model (with sedimentation rate) would be most useful for the reader in evaluating core chronology and integrity.**

We have included a diagram.

**Typos/other minor Qs**

**4691 L2 Change important to key? Reads a bit oddly**

Done

**4692 L14 Heinrich events/stadials also seen further afield in tropical Africa e.g. Tanganyika (e.g. Tierney et al., 2008)**

Argument for global expression is now supported by this reference – thanks.

**4693 L9-10 This part of sentence reads awkwardly**

Text has been reworded

**4693 L3 Why have reference for lake altitude? Surely not debated?**

Ref is removed.

**4693 L13 Need reference for water budget/hydrology**

Done: Matzinger et al. 2006

**4693 L17 Rephrase, e.g.: “it is undergoing [ref] or may undergo a switch. . .”**

Yes – edited

**4696 L13 C2 should have a reference – see suggested reference in C2 manual (...(..)Juggins, 2003)**

Thanks– added.

**4697 L16 “. . . rare” – sentence should be re-phrased?**

Done

**4697 L21 add “in”**

Done

**4701 L4 Add “seen”**

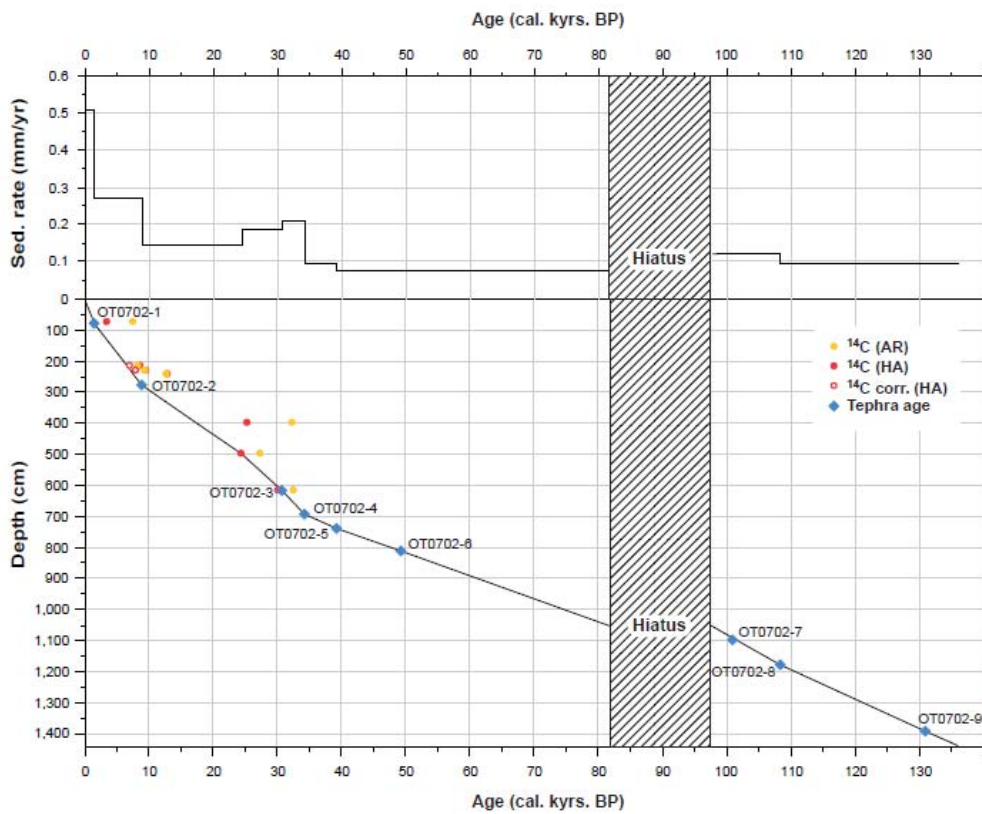
Done, in the midst of other editorial changes.

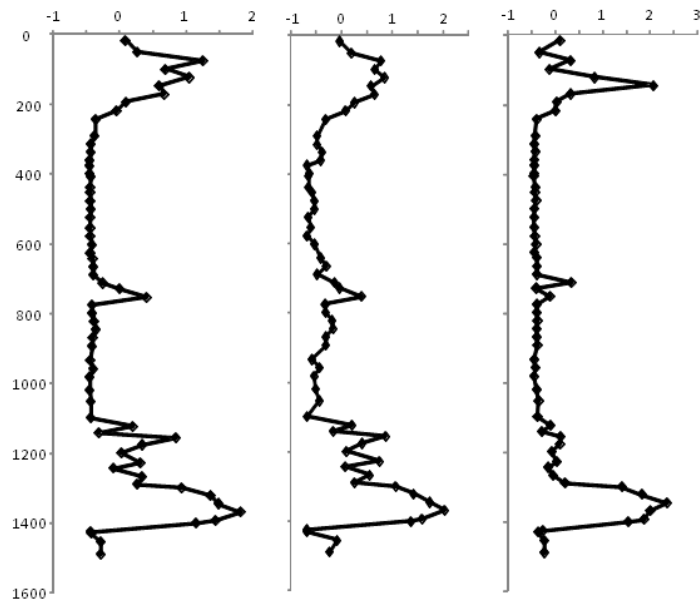
**4701 L19 “respond”**

Done

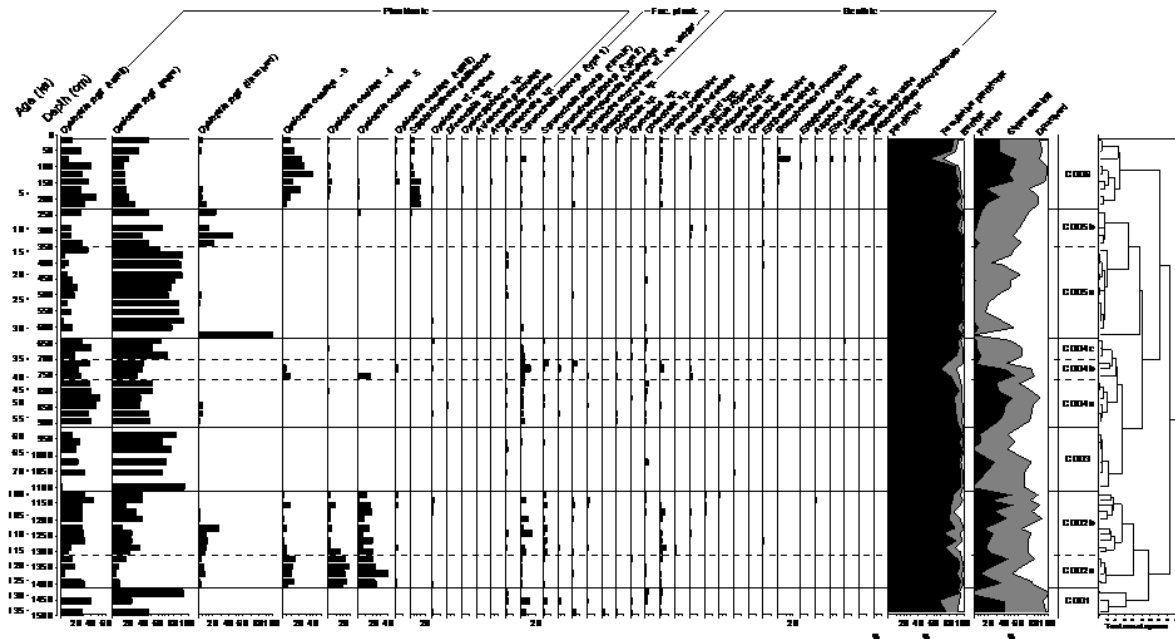
**4701 L20-22 Possibly with ostracods etc from a littoral core – though these might of course be rare in a glacial (more chance of finding these with a shallower, littoral core however)**

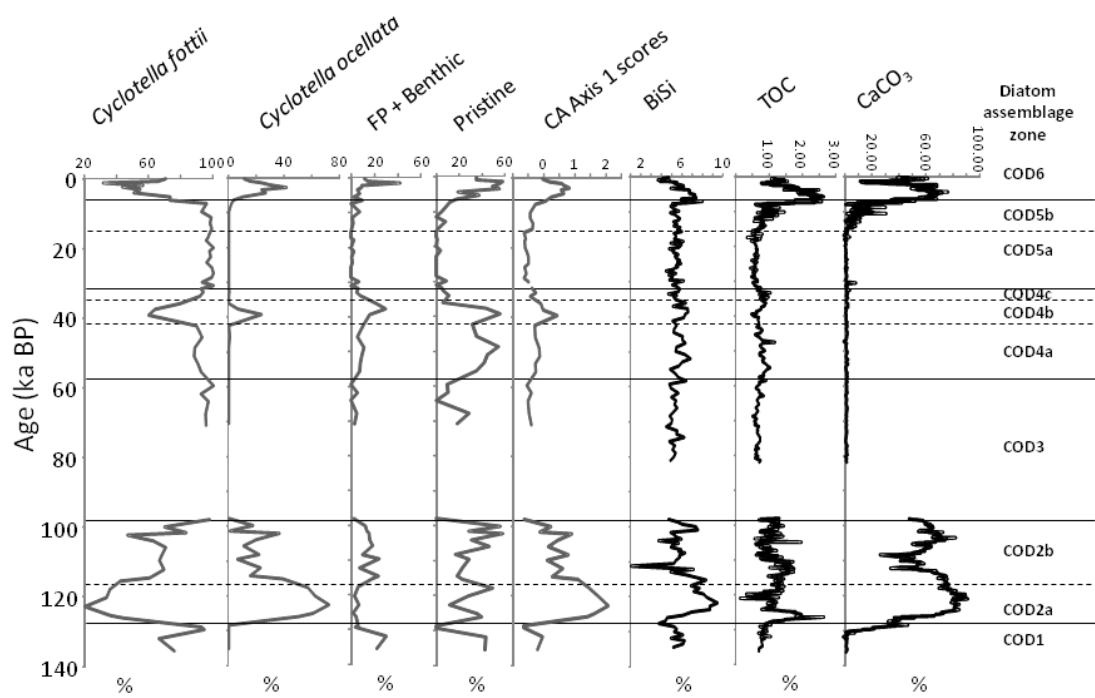
Yes, ostracods are absent in glacial phases – I’ve added a Schwab pers comm., pending publication.



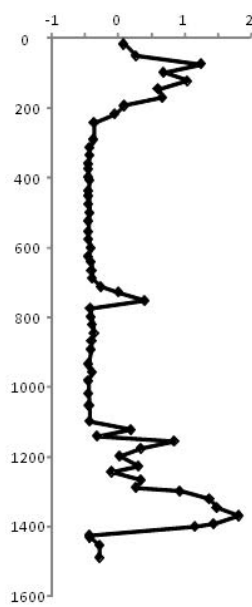


**Fig. 2: post-referee version.** Comparison of Axis 1 scores for correspondence analysis (CA) of the merged UK data-set (*Cyclotella fottii* and *C. ocellata* as single species), the UK split data-set (minus indeterminate dissolved centres of *C. fottii* and the Macedonian split data-set.

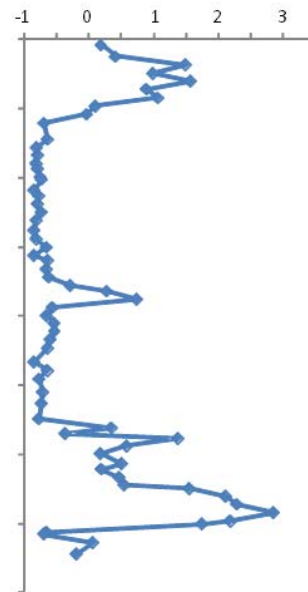




UKmerged: CA

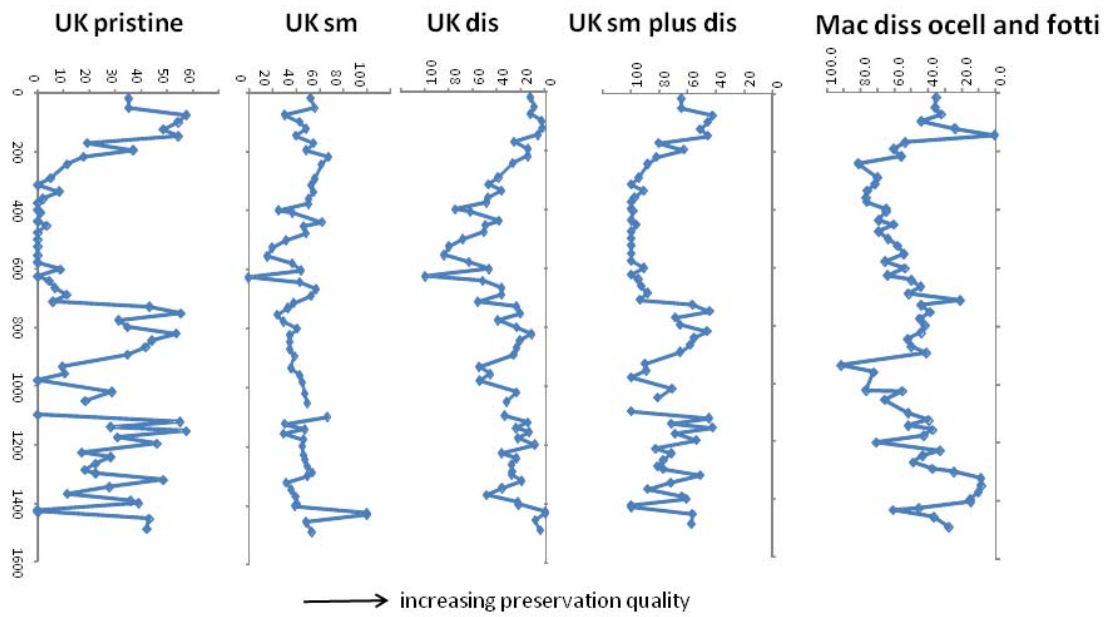


UKmerged: PCA



Comparison of Axis 1 scores for CA and PCA of the UK merged data-set; sample at 72.4cm has a diverse range of rare benthic taxa, the influence of which is exaggerated in PCA





*For response to referees:* comparison of percentage data for different dissolution classes in the UK data-set, compared to the Macedonian 'dissolved' [unidentifiable for detailed taxonomy] class. 'sm' = marginal striae missing; 'dis' = dissolved centres