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Interactive comment on "Ecosystem regimes and responses in a coupled ancient lake system from MIS 5b to present: the diatom record of lakes Ohrid and Prespa" *by* A. Cvetkoska et al.

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Cvetkoska et al: Ecosystem regimes and responses in a coupled ancient lake system... Review by Dr Jane M Reed Many apologies for the delay in submitting this review; it was unavoidable. I am working on the original submitted manuscript line and page numbers, since the printer-friendly proof-read version of the draft manuscript seemed to be that of another paper. General comments This paper builds on some of the previous research on ancient lakes, Ohrid and Prespa, hydrologically-connected tectonic basins in the SE Balkans which are characterised by a 25% inflow from the relatively shallow, nutrient-rich Lake Prespa into the southeast of deep Lake Ohrid via

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subterranean springs. The authors are part of a collaborative team working within the International Continental Drilling Programme project, SCOPSCO, which is led by Cologne and aims ultimately to understand the influences of environmental change on evolution in these systems. As in other ancient lakes, the high degree of endemism and biodiversity qualifies them to be regarded as of global importance for evolutionary biology. Multiple Late Quaternary sequences have been retrieved from the lakes over the last decade and, with the successful retrieval of a ca. 1.2 Ma sequence from Ohrid, they are also regarded as of global importance for palaeoclimate research. The authors state that they aim to understand the 'panarchy' of the linked system, a theoretical concept for understanding complex ecosystem dynamics and multiple forcing functions on a variety of spatial and temporal scales. A central tenet of the theory (Allen et al. 2014) is that change can be driven from the bottom up, internally, but the paper does not deal with internal ecological dynamics. It may be that they consider the influence of inflow from Prespa as internal to Ohrid but it is not clearly explained. If panarchy is to be the central theory, it would be helpful to justify more clearly the use of various theoretical terms such as adaptive cycles and ecological collapse. The adaptive cycles described in the abstract are those of glacial-interglacial climate change, yet they are stated as driven by climate, tephra impact and/or human influence (ln 20) and the paper goes on to try to consider shorter-term variation. The paper presents multi-proxy results from the last 92 ka Prespa sequence (previously published), comparing it with a 92 ka sequence from the top of the most recently-drilled sequence 'DEEP'. Prespa is already published. The authors do present a new set of diatom and geochemical data from the Ohrid DEEP site, at higher resolution than the preliminary study of the last ca. 134 ka by Reed et al. (2010) and Cvetkoska et al. (2012), but it is a major omission to have ignored the publication by Zhang et al. (2015) of the Lini sequence. The great scientific rigour of SCOPSCO is that papers have built successively on improvements in understanding in the current published SCOPSCO literature. The Lini sequence provides a higher resolution and more continuous sediment record than the DEEP; more importantly, it also supercedes the Reed et al. (2010) over-simplistic inter-

pretation of clear and consistent glacial-interstadial-interglacial temperature influence on the diatom flora. Instead, the in-depth Zhang et al. study highlights the complexity of diatom response, incorporating complex interactions with catchment dynamics, nutrient availability and mixing regimes. Rather than repeat some of their comments here, Cvetkoska et al. are urged to read the referees' hearty critique of this paper. In spite of highlighting complexity, all urge yet more caution in interpreting temperature influence. Cvetkoska et al. simply base their interpretation on previous assumptions which are now proved invalid (and please see further comments re interpretation, below). Thus, the palaeoclimate interpretation here is unnecessary and does not add to current understanding. Instead, the major potential strength of this paper is that it does provide the first detailed comparison between sequences from the two lakes. The paper would be strengthened greatly if a single hypothesis were raised – that inflow from Prespa might influence the water chemistry and diatom ecology of Ohrid. This is where the originality lies and – see comments below – this aspect could be approached in more depth. As commented below, an understanding of the possibility that Prespa might influence Ohrid is essential if the earlier phases of the DEEP are to be interpreted coherently in terms of climate change. Specific comments Abstract The first sentence does not make sense – panarchy and interactions of what? The lake system is the oldest, but please reword this sentence because it sounds as if it is also stating it is also the 'most' hydrologically connected. The English grammar of the abstract needs checking throughout. In 18 could read 'we compare palaeolimnological diatom and selected geochemistry data from the Lake Ohrid 'DEEP' site with equivalent data from Lake ... In 20 As noted above, this sentence does not make sense. There is a clear long-term influence of climate change on both lakes (already understood and cannot be an original aim of this paper) but on a shorter timescale there is also potential for complexity to be introduced by other forcing functions. In 22 please define regime shifts. Do you only consider an MIS stage as a regime? Since shorter-term variability is referred to in the paper this needs to be clarified. In 23 mixing of present and past tense In 26 – see comments below. Ohrid is not simply buffered. In 26 – see comments below. This

statement needs justifying in the paper. The Lini study does not show the pattern suggested on page 2 ln2. ln 3. – see comments below. Please check English. How do you define ecological collapse? There is no discussion of feedback (positive or negative) in the paper – please delete. Should read 'may confer' rather than 'probably confine'? ln 9 should read ecosystem interactions ln 12 Better to say continental, since terrestrial is a term usually confined to non-aquatic ecosystems. Also, there is no real evidence for 'evolution' of structure and function; a fundamental assumption of the palaeoecology is that modern ecological relationships can provide insight into past change, based on principles of uniformitarianism. If structure and function evolves, our ability to carry out palaeoecology is invalidated. The simple term, change' might be better ie 'function of continental ecosystems is known to have changed on a variety of timescales during the Quaternary, driven primarily by the influences – direct or indirect – of climate change. The nature of ecological collapse needs clarifying – how does it differ from change? Explain omega phase and justify its relevance as a concept in this study. ln 22 it is not just the intensity of external forcing. The character of ecological response can vary between different types of water body – as we have shown. p3 ln 11. Needs clearer definition – the inflow from Prespa to Ohrid is logically an external influence. There is no influence of Ohrid on Prespa, so generalising this as being the internal drivers is rather loose. ln 4. Matzinger only argued that the influence may occur. As noted above, this should really be the main idea to be explored in this paper. ln 14 should read 92.0 ka rather than 91.0 ka. You must allude to Lini-Zhang in this paragraph and include the site on the map in Figure 1. As a convention, it is normal to order papers from ongoing research in date order rather than alphabetical – please check the whole manuscript. ln 24 there is no hiatus in Lini and it is a higher-quality sediment record than the DEEP Holocene. This must be included. page 4. ln 4. the paper does not seem to add understanding of Ohrid's internal dynamics – please reword. ln 6 poor English. ln 10 introduce and define these theoretical terms more specifically in terms of the limnology of the two lakes, or simplify the terminology. ln 14 environmental setting. This is well written and with good detail. Would be good to cross check the two site descriptions so

that they have the same order of data and refer to the same terms e.g. mean annual temperature. In 24. Again, Matzinger suggests this may have occurred. In 29 fewer. 1971 is a very old paper – is there any more recent evidence to support the statement. page 5 ln 2. Prespa is relatively shallow – much deeper than many others. lines 10, 11. Lake-level change should be hyphenated. In 13 poor English. In 18 core recovery. Maybe do not need to give detail of the full coring – could shorten this section to focus in more depth on the age model for the last 92 ka. In 25 should read based In 27 should read Lisiecki. In 27 you need to include the resolution of sample analysis for Prespa, since it is very important to have similar resolution to allow you to consider patterns of change. Even if it was not intentional to achieve this, it is important and should be included specifically in the Methods. If 92 ka is 37.5m in Ohrid and 17.7m in Prespa, the x16cm Ohrid and x8 cm Prepspa actually do match up. But some comment should be made somewhere about the apparently higher sediment accumulation rate in Ohrid. This is counter-intuitive since an oligotrophic lake has a slower SAR than a meso-eu lake. What might be the reason for this? The methods section needs a data analysis section for both sites. Need to justify the use of DCA in prespa and PCA in Ohrid. What is the gradient length of Ohrid data? Since you are comparing degrees/amplitude of change, you should also state that you have maximised apparent variance by running ordination on species morphotypes at a sub-species level. For this study, rates of change analysis might be a useful statistical approach? page 7 ln 11. Wrong order of citation – should be Reed et al. 2010; Cvetkoska et al 2012 and Wagner.... In 13 it is not really imperative – reword as 'may be useful'. In 17. This really is not true – we are only beginning to understand the complexity of diatom response in Ohrid. You must cite Zhang et al (2015) here, particularly since the counting of very dilute slides has revealed the presence of C minuscula at the start of the Holocene – could this have been missed in DEEP counting? In 22 You have taken on our Zhang et al approach of splitting taxa to epi- and hypolimnetic. This is an unusual approach so you really must cite the paper as having the idea, to avoid the appearance of plagiarism. In 26 if benthic and FP taxa do not add much, maybe summarise rather than display them

in the diagram. In 28 Reed was referring to MIS2 – reword since their appearance in the Holocene is certainly not early ice out. You do not mention that the DEEP results have a surprisingly consistent abundance of FP compared to Co1202. They are not just present in MIS3, so the preliminary interpretation is no longer valid. Why are there more FP??? Page 8 ln 3. In presenting results of PCA you should also include a table of eigenvalues, comparing with Prespa. What proportion of variance is included in Axis 1 and Axis 2. The interpretation of PCA Axis 2 as productivity is not well justified; the multiple ocelli morphotypes plot close to the centre of the diagram and we have no evidence that they indicate higher productivity. In Prespa, we could interpret DCA Axis 2 as productivity on the grounds that there was a clear additional gradient of variation, such that plankton-dominated phases were sometimes dominated by eutrophic taxa, indicating that P:B ratios were not simply lake level. Here, you need to explore what is driving the full axis 2 variation – there are two long branches and perhaps they are driven by rare occurrence of benthic taxa. If so, you should rerun the PCA on e.g. taxa present at 1% and explain what you are doing. If the proportion of variance in Axis 2 is actually very small, then it should not be viewed as a major gradient of variation. The diatom diagram includes the proportion of dissolved fottii, and the fottii F index. The former should be the inverse of the latter – please clarify how you calculated the F index. Ln 13 Ecosystem dynamics You show good caution here in arguing that rapid change events in Prespa cannot be matched exactly with Ohrid due to uncertainty in the age model. However, if you aim is to track whether P has a major influence on O, you can argue that the lack of similar abrupt peaks in Ohrid does indicate that Prespa does not have a simple direct influence on the ecology of Ohrid. I would include this in carrying out a general pattern comparison between the two lakes. Page 9. The attempt to interpret palaeoclimate based on older ideas of diatom response is not necessary, and not well justified. Minuscula dominates Lini in the early Holocene in a phase of nutrient limitation, for example. What evidence would you have to argue it is more windy? What you can do is to argue that the fluctuations during this time in Prespa, do not find an equivalent in Ohrid. MIS 5a. You need to include A. granulata on the

diatom diagram. You mention briefly a small peak in A granulata in Ohrid, which correlates with the major peak in this species in Prespa! In an ultra-oligotrophic system such as Ohrid, this is big news and probably does provide evidence that in extreme phases Prespa CAN have an influence on Ohrid. You should think about whether it is simply the transport in of diatoms from Prespa (ie a taphonomic effect) or whether it represents a brief phase of nutrient enrichment in Ohrid. Throughout this section, I would suggest removing allusion to climate change in Ohrid, and restructuring to start with description of Prespa dynamics. This is logical if you are trying to assess the influence of Prespa on Ohrid rather than vice versa. MIS 4 Suggest you do a more detailed comparison with Prespa; do low lake levels in Prespa have any influence on Ohrid? Why do you have ocellata here, when it was absent in Co1202? MIS 3 It has already been suggested that the palaeoclimate interpretation would be better taken out. As an example of uncertainty, the minuscula dominance is interpreted here as improved light/temp, but earlier as low light. Certainly, there is no evidence in the Ohrid diatoms for moisture availability (ln 14). In the spirit of comparing the two lakes, make more of the ideas in the paragraph starting on ln 20, but starting with Prespa. MIS2 – you will presumably argue it is impossible to test the influence of Prespa on Ohrid here since they are both suffering major change to LGM state. MIS1. Please remove; this is a less complete record than Lini. Lini is the paper to refer to; there are some surprising discrepancies between DEEP and other cores e.g. high concentrations after 2.5 ka in spite of high SAR occur in Lini, whereas the DEEP appears to have low concentrations. Instead, you might look at degrees of variance in ordination. Since you choose to maximise apparent variance in Ohrid, this means that if the variance in Ohrid is lower than that of Prespa, it is a real phenomenon. You could look at the whole record in these terms. This might support the argument that Ohrid is somehow more buffered, as a deep lake. At present this statement, which appears in the abstract, is not very well justified. 6.1 adaptive cycles These sections need an English check Page 16. It is not surprising that the lakes respond to glacial-interglacial magnitude of change – I would shorten this section. The dominance of benthic/FP in Prespa is a function of

lake level. There would have been plenty of benthic taxa around the shores of Ohrid – it is simply that they do not reach the coring site. The main point in this section is that glacial phases do not cause the complete loss of diatoms. You compare with Baikal and Hovsgol. These are in Siberia rather than the Mediterranean, so it would be interesting to discuss contrasting climate characteristics here. Page 17. Resistance and resilience are interesting concepts. You do not necessarily have simply productivity data in diatom concentrations though – see refs comments on the Lini paper. This section could make good use of variance in diatom data, as suggested above. You may even take out the MIS sections and combine into a focused, strong comparative discussion of patterns of variance in the two lakes. Ln 24. Four samples is not enough to provide a rigorous comparison of response to a putative Medieval Warm Period; if you did so, it should be in a results section rather than at the end of the discussion. You might mention this earlier if you wish to retain it. Returning to the comparison, a more in-depth assessment might pull out other periods during which Prespa could have an influence beyond parallel response to external forcing functions? If it does not, that is a very important finding. This means that we can have confidence in interpreting earlier phases of the DEEP in terms of climate, removing one of the many possible aspects of complexity in interpreting response. This is in itself sufficient for a well-focused paper. The ending on a note of ecological collapse is rather weak. I'd suggest it ends on a note of implications for future Quaternary palaeoclimate analysis, since you have not dealt with recent environmental change here. Additional reference: Zhang, X.S., Reed, J.M., Lacey, J.H., Francke, A., Leng, M.J., Levkov, Z., Wagner, B. Complexity of diatom response to Lateglacial and Holocene climate and environmental change in ancient, deep and oligotrophic Lake Ohrid. Biogeosci Discuss 12, 14343-14375, 2015.

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