

Following major revision, the manuscript is much improved. There is now a well-defined focus on (1) whether hydrological outflow from Lake Prespa has an influence on the dynamics of Lake Ohrid and (2) a comparative discussion of resistance and resilience of the two lakes. The manuscript has been checked very carefully for spelling errors (although note bellow rather than below, and Panagiotopolus rather than Panagiotopoulos in one instance), but would still benefit from editing of English grammar.

The authors have now acknowledged that the notion of a simple temperature response in Ohrid is no longer viable, although they maintain the idea in discussing palaeoclimate forcing on a millennial timescale. This is still controversial (as is the interpretation of *C. minuscula*, the unpredictability of which is indicated by its weak correlation to geochemistry data in Table 3), but since the complexities of climate response are not the main focus of the paper, it may rest. Zhang et al. (2016) deal with this on a shorter timescale; it is reassuring to see acknowledgement of this paper, which had been discussed with the group previously. Interpretation of geochemical proxies is now justified well.

The concept of panarchy is retained in the paper. The authors essentially apply the rather complex terminology to assess the resistance and resilience of the two lakes. The results fit with our understanding of the differences in response thresholds between the two, with Ohrid apparently buffered both against major influence from Prespa, and from rapid, extensive response to shifts in climate. The discussion acknowledges clearly that the more dramatic shifts in diatom species assemblage composition in Prespa link more to the impact of shifts in effective moisture, resulting in lake-level change. Acknowledgement of the importance of lake-level change needs also to be incorporated into the Abstract. My main comment requiring addition of text is that, if you maintain the focus on shifts in ecosystem regime, you need to add a short paragraph to the discussion in which you assess critically the degree to which the stratigraphic diatom data do actually record a real shift in ecology. This needs to be disentangled from the effects of physical changes in habitat distribution with lake-level change (in Prespa, rather than Ohrid), such that assemblage composition would change at the coring site, but no real ecological change may have occurred. Where planktonic species composition changes, this hails ecological change. Where there is simply a relative increase or decrease in benthic taxa but no change in planktonic dominants, this might not represent a 'regime shift'.

The PCA interpretation (p9 line 10) is still rather unclear since taxa with high/low scores on Axis 2 are not incorporated into the interpretation. However, the reliance on this as a productivity gradient is no longer prominent in the interpretation.

p11 ln 5. This sentence is a response to the suggestion that the lack of abrupt, major peaks in Ohrid during the earlier record provides useful corroboration that Prespa's dynamics do not drive directly those of Ohrid. The sentence would be better incorporated specifically in discussion of the relevant part of the sequence.

A comment not addressed in the response is the suggestion that the small peak in *Aulacoseira granulata* in Ohrid, which seems to correlate with the major peak in this taxon during MIS 5a in Prespa, should be given attention. It may indeed be evidence of some influence of Prespa during extreme low lake levels.

p12 ln 14. This sentence is not clear. A small increase... correlates with rather than marks?

Figure 2: *Cyclotella minuscula* spelling