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Interactive comment on "Differential resilience of ancient sister lakes Ohrid and Prespa to environmental disturbances during the Late Pleistocene" *by* E. Jovanovska et al.

Anonymous Referee #1

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General Comments This paper is a contribution emerging from drilling at Lake Ohrid and examines the response of Lake Ohrid and Lake Prespa diatom communities to the H4 event 40-38 ka (which they categorize as a press event) and to the deposition of the Y5 marine tephra layer (viewed as a pulse event) at 39.6 ka. The conceptual framework is to use this analysis as a measure of ecosystem resilience, and, in general, I think the assessment of the diatom response to these disturbances is interesting.

The characterization of Heinrich events as press events seems somewhat peculiar to me given that in the paleoclimate literature Heinrich events are viewed as relatively short term events, and H4 in particular is characterized as being abrupt and extreme. Admittedly Heinrich events are of longer duration than a volcanic eruption, but I'm not

convinced that the pulse/press event characterization is really necessary or useful in this analysis. I think the paper would be better if it simply relied on contrasting the abrupt impacts of tephra with the longer-term impacts of climate variation, without the jargon.

The paper concludes that the diatoms do show an abrupt response to tephra deposition but do not respond to the H4 event (but this latter statement is not consistently supported by the data - see my comments below). Differentiating the onset of these two "events", of course, hinges on chronology – as the Heinrich event precedes the tephra deposition by just 400 yr. Yet no comments are made on the errors associated with the chronology, so it is not clear how well supported this statement is. This should be discussed early on in the manuscript. In general, the discussion of the impact of climate variation associated with Heinrich variability is inconsistent. In addition, for Lake Ohrid, the sampling resolution prior to the onset and after the cessation of H4 is very low, so it is difficult to make a clear assessment of the impacts of H4, because of the changes in resolution. In general, I think the discussion of the impacts of climate need to be revised and qualified in many places in the manuscript – see my comments below.

I also think the sampling resolution is a significant problem in the discussion of the extent to which the floras recover their pre-disturbance state, which is not acknowledged adequately in the discussion of the data. Based on Figure 3, for example, there are really only two samples above the gray Heinrich layer – how does one know what is signal versus what is noise?

Specific Comments Page 3, Lines 13-15: This sentence should be less definitive. In both lakes, speciation patterns have been inferred for only one faunal group– hence there is not sufficient data to make generalizations, such as "the evolution of their species". And, at least in the case of Lake Titicaca, there is no demonstration of extinction in the paper that is cited – just speculation about extinction. So the sentence as written is not correct. I suggest you say something like, "the evolution of some faunal groups was influenced by massive environmental disturbances, leading to (near-) desiccation of the lakes."

Furthermore – both Lake Malawi and Lake Titicaca have long diatom record – why doesn't this part of the introduction summarize what the diatom stratigraphy of each of these two lakes suggests about diversification patterns (rather than using the faunal records)? Or alternatively, it might be more appropriate to summarize what is observed in the diatom records of Lake Baikal and Lake E, for example, which arguably are more similar to Ohrid and Prespa than the two tropical lakes.

Page 7, Line 10: It is not clear what is meant by "Until today." Recently? Please change the wording to be clearer.

Page 13, Line 10: As I indicated above, how good is your age model? How much error is associated with your characterization of the onset of H4?

Page 13, line 17: I think the basis for saying the Prespa community recovered is rather weak given that there are only 2 samples in the upper part of the diagram, and they are very widely spaced.

Page 13, Line 25: Most of the prior discussion has centered on the impacts of the tephra deposition – Heinrich events are mentioned only briefly – so I think it would be better to start the discussion with a focus on the major theme (tephra deposition) – and later move into assessing how climate affected the flora.

Page 14, Line 23-26: You start the Discussion section by saying that the Heinrich events had little effect on the diatom community – yet here you say that there is increased representation of benthic species, likely because of mixing at the onset of H4. The two statements are inconsistent.

Page 15, line 24: Do you mean that climate may have delayed the recovery (rather than prolonged)? And again, saying that climate variation associated with the Heinrich event may have affected the rate of change in the diatom community structure is inconsistent

with your statement that Heinrich events had little effect.

Page 17, lines 1-2: Again, here you are concluding that climate affects the diatoms. Also, here and in several other places, you conclude that climate likely prolonged the recovery of the diatoms community from the volcanic ash deposition. The basis for this conclusion is not clear – is it just that you expected a rapid recovery yet it is prolonged? Or is there some more distinct evidence that climate plays a role? Please articulate the logic.

Page 18, line 20: I think the variable sampling resolution, particularly the coarse resolution in some sections of each core, imposes some serious constraints on the ability to differentiate real trends versus sample to sample variation. This section should acknowledge this.

Page 19, line 6: The paper by Spanbauer (2014), which discusses long-term responses of diatom communities to perturbations, would be relevant to the discussion here. Spanbauer, T.L., C.R. Allen, D.G. Angeler, T. Eason, S.C. Fritz, A.S. Garmestani, K.L. Nash, J. R. Stone. 2014. Prolonged instability prior to a regime shift. PLoS ONE 9: e108936, doi:10.1371/journal.pone.0108936

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