

Interactive comment on “Synoptic climatology and recent climate trends at Lake El’gygytgyn” by M. Nolan et al.

Anonymous Referee #1

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Overall, this paper was extremely tough to review. It is real dense to read, and I think that as written, it won't be broadly appealing to readers of *Climate of the Past*, paleoclimatologists, nevertheless even most synoptic climatologists. Therefore, I think it needs to be substantially revised before consideration of publication. Below are some general comments, not in any particular order.

1 – One needs to be a close follower of the work by Cassino et al and Nolan to really understand and appreciate their work. After reading the other cited works by Cassino et al. I think that the synoptic climate research is of very good quality and very highly admirable, but the general design in relating to Lake E and clarity needs to be further improved. The authors cannot assume that readers [even many synoptic climatologists] can easily grasp and understand SOM from Section 2 (and figures 4-5, 10, etc.)

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quickly. Perhaps the solution is that Section 1 be further expanded in background, writing more clearly for a general audience.

2 – The “companion” paper of Nolan is important for obvious reasons in this paper, and it needs some background on it more extensively too, since that data is the framework for devising synoptic patterns and linkages.

3 – Throughout the paper, the numerous acronyms make reading very confusing. The authors should consider writing out more plain text. Figure 1 is too tough to easily read. I know Cassino et al have successfully used this style in previous work, but I just think that the visualization is too tough here. Also, in each synoptic map, Lake E's specific location should be highlighted to facilitate synoptic interpretation.

4 – The authors used only sea-level pressure data. This is a major constraint, as processes at sea-level only explain so much for the Eastern Siberian region with lots of cold air sinks in winter, etc. The upper-air would likely capture additional processes. The synoptic size of the study area is also a constraint. It is the same size as Cassino et al did in their Alaska work. However, in eastern Siberia, the synoptic patterns from areas to the west and south need to be expanded to get a full view of what is really going on.

5 – I cannot expand much here other than to say that I found sections 3 and 4 very confusing and a tough read. A typical synoptic climatologist reader is used to seeing how much variance is explained, and different patterns are important in different seasons. Is there any way to provide some simple tables/graphs for summary?

6 – Figures 2 and 8 show interesting results from NNR but it is just tough to see how much the synoptic aspects are addressed, which I assume the paper is really trying to do.

7 – Section 7 rambles quite a bit and is obviously kind of different than the rest of the paper. The authors need to touch much more on the important paleoclimate issues,

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controls, and forcings, particularly from climate model simulations (with appropriate citations). Their synoptic approach could be a useful bridge from local scale to the models.

8- the Bibliography is very poor in regards to work not by Nolan and Cassino, and in terms of the appropriate synoptic and paleoclimatic literature.

I think that I have commented enough that the study is worthwhile but would need a very serious revision and analyses of more data before publication.

Interactive comment on Clim. Past Discuss., 8, 1485, 2012.