

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

Anonymous Referee #2

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General Comments

This is an interesting paper, applying the SOM technique to investigate the synoptic climatology at Lake El’gygytgyn. Lines 5-7 on page 1491 demonstrate an important value of the study: “Our methods will hopefully make clear what teleconnections exist between Lake El’gygytgyn and the surrounding domain and how to exploit them, such as through comparisons with other paleoclimate sites within the domain.”

As one not intimately familiar with the SOM technique, reading the paper required serious concentration. To be broadly readable, and for the findings to be useful for a wide audience, improved organization would be helpful. I also suggest that the perspective of an arctic synoptic climatologist is obtained to insure that SLP anomaly data is the most appropriate field to assess both the modern situation, as well as how relevant

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modern circulation is to that of the past.

I found no location map of any sort in this manuscript, a major oversight despite the paper being part of a special issue. This is not a deficiency that over-worked reviewers should have to deal with. Figures 1 and 3b really need to show the lake location on every pattern, given the subtleties involved with the delineation into 35 different patterns. Less significantly, misspelling of the study location name in Fig. 3a is difficult to excuse. Reviewing manuscripts is a time-consuming process, and manuscripts sent out for review with such blatant problems distract from reviewer’s ability to conduct a proper review (best case) or even negatively bias the review.

In the introduction, the authors state clearly that this work was inspired by, or a value-added product of, the Cassano et al., 2011 paper. Presumably then the spatial domain for that paper and this manuscript are identical, yet without plotting the location of the present study site this remains uncertain. Are they close enough that the analysis of the same domain is appropriate, or does the study center need shifting to the west to better represent circulation at Lake El’gygytgyn (esp. patterns involving circulation further west)?

Perhaps greater emphasis should be given to lines 23-26 on page 1499, in the abstract and concluding thoughts, as this provides important, solid evidence for anthropogenic warming.

Looking way back in time, the authors seem to clearly recognize and acknowledge the problems of considering how relevant modern patterns are to climate over millions of years. I am very skeptical about this, yet as they state, the modern period is the “only direct measure we have”. I suggest considering the inclusion of this specific concern in the abstract, perhaps rather than simply stating that “we conclude with a discussion of how these results may be relevant. . .”. Perhaps a subheading within the manuscript body could address just this issue?

With study, the colors illustrate the patterns and pattern frequency changes adequately.

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There isn't much hope of seeing these when printed in black and white, which demonstrates that publication in an open-access journal such as CP is a sound idea.

Specific Comments 1. For Fig. 4, and the statement on p. 1491, line 26, do the figures show that the freeze (thaw) seasons are long (short) by the sum of the text numbers? I assume so, but this isn't entirely clear. 2. Fig. 8 discussion on p. 1496: Please clarify the intent of lines 8-11 (e.g., "remaining NDD from the winter season shows essentially no jump or trend. . .")? There look to be "trends" of varying lengths and timing in this figure, with fall maybe trending upward from 1980 – 2006, winter from 1990 – 2000, spring from 1985 – 2002, and summer from 1998 – 2007. While I don't doubt this warming, a more convincing case needs to be made that simultaneous change happened primarily in fall and spring. Fig. 9 helps, although resolving patterns of change via the numbers over several pages is difficult. 3. p. 1496, line 3: Discussion here wraps up a nicely written part of the paper. I suggest that degree-days be calculated for each true cold season rather than on the basis of calendar year. Although differences may be small, numerical convenience is inadequate justification.

Technical Corrections 1. Fig. 2 (A) temperature scale is missing 2. Fig. 7 (A): use actual year on x-axis 3. Fig. 9 would benefit with a caption more like Fig. 10, which I guess would read something like "Text shows annual changes in degree days between 1995–2009 and 1961–1975, colors show mean degree-day warming over the entire period 1961–2009"??? Looking simultaneously at the two patterns is tough. . . 4. Be consistent throughout paper, either NCEP/NCAR or NNR

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