Response to Reviewers of "Synoptic climatology and recent climate trends at Lake El'gygytgyn" by Nolan, Cassano and Cassano.
11 Nov 12

As with most papers, there were three types of comments from reviewers: 1) broad-scale comments about suitability for the journal, 2) discussions on the science itself, and 3) minor editorial suggestions to improve clarity. As we describe below, we acknowledge and agree with most of the concerns expressed in type #1, we have changed the text to address all comments in type #2, and we have made nearly all changes suggested in type #3. We lumped together similar broad and science questions from the authors for type #1 and 2, and addressed editorial comments individually by reviewer.

1) Broad-scale comments about suitability
All authors expressed that the paper contained useful, interesting, and scientifically valid results. Opinions differed on suitability for CoP, the degree of detail required, and whether major structural changes are required.

Reviewer #1 was most critical as to whether the paper should be published at all in CoP or if so that it required major revisions: "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."

In general, we agree that this paper may not be the best fit for CoP, but it is an excellent fit for with the Lake El'gygytgyn special issue. Similarly, we agree that this paper may not be broadly appealing to regular CoP readers or to other climatologists, but our reply to this is simply that this paper was not written to obtain broad appeal, it was written specifically to appeal primarily to the ~50 people involved with the Lake El'gygytgyn science project and maybe only truly useful to 3-5 of them, and since we have been involved with this project since its inception we have a good feel for what will be useful to that group and what will not. As to the paper's "density", we agree it is a very dense paper, but this is also by design, especially considering how expensive page charges are and we do not see density itself as a negative thing; Shakespeare's writing was incredibly dense, but this does not make it bad, and while we make no claims that our writing and figures are even close to being on par with his, we do believe it is something to aspire to. The densest aspects of the paper are the figures -- those interested in lake weather could spend hours or days extracting useful details from these, our text is mainly designed to help readers use the figures in whatever ways are most useful to them, we only paint enough of the broadest brush strokes here.

Reviewer #1 was also quite critical about his background on SOM as a typical CoP reader being insufficient to understand and review this work and therefore suggesting it needs to repeat much of what is contained in previous papers. Reviewers #2 and #3 also

noted the background and time required to get the most from the paper. Here are their comments:
R#1: "One needs to be a close follower of the work of Cassano et al and Nolan to really understand and appreciate their work... I think that the synoptic 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) quickly. Perhaps the solution is that Section 1 be further expanded in background, writing more clearly for a general audience."
R#2: "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."
R#3:"At initial glance the manuscript appears highly technical and overly complex. Clearly one cannot easily jump right into the middle of text and figures unless already well familiar with the SOM methodology. But beyond this and similar limitations, the authors do well in building through each set of results from the initial 35 SOM maps to the final attribution section. Results described in the text are supported by the graphics. As with many journal articles, the appeal of the technical details will be limited for many readers of CP. Graphics are not easily understood without reading through portions of the text. But I see no obvious need for a major alteration to the manuscript structure."

Again, we agree with most of these observations. Additional text has been added to the introduction for readers not familiar with the SOM technique which includes additional information on the SOM methodology, what the output represents, and details from our previous work which the current work references and expands upon. Our focus in the paper, however, remains explaining the results of our SOM analysis rather than the analysis itself. Therefore we have also tried to elaborate on example uses of the analysis and add more descriptive figure captions.

In terms of the paper's complexity, we did not include most of the technical derivations to spare the reader what he or she likely has no interest in, and provide non-climatologists with enough information to make the discussions understandable so that the average member of the Lake El'gygytgygn science team could get the jist of our thought process without needing to really understand the details. For example, we point out elementary but non-intuitive concepts like low pressure systems spin counter-clockwise. We added unique ID numbers (1-35) to the SOM plots rather than the conventional (column, row) notation. We introduced degree-day analysis here for the first time in a SOM climatology paper because this is a useful metric for terrestrial processes that most climatologists are not interested in. Plus most of the text in sections in 3, 4, and 5 are verbal descriptions of the figures that capture the most important elements of those figures, so that novice climatologists are not forced to interpret (or even look at) the figures for themselves to get the important points. These descriptions hopefully also provide enough information to new readers such that they have the tools to dive deeper into the figures so that more subtle details can be extracted, but it is true that this does take some effort. This information is written at a much lower level of jargon and prior knowledge than most previous papers on SOM analysis of climate.

So while we agree with the reviewer's observations, we disagree with Reviewer #1's impression that a major revision is required, as we believe these difference stem from our differences in intended audience and their interest level, though we have tried to improve clarity and organization further in response to these suggestions, using the philosophy just described. We have also expanded the figure captions, such that one could get more out of them without reading the text.

Reviewer #1: "I think that I have commented enough that the study is worthwhile but would need a very serious revision and analysis of more data before publication."
Reviewer #3: "The authors make clear the earlier work that supports the use of SOM for achieving stated objectives. They present a synthesis of their results and structure of the paper is a logical orderly flow. I see no obvious need for major alterations to the manuscript structure. However, the manuscript can be improved."

We believe that our differences of opinion with Reviewer #1 as to the changes required here stem largely from our difference in intended goals, and we agree with Reviewer #3's analysis and believe we have improved the manuscript. However, whether substantial changes and more data are required is a decision that the editors will have to make.

2) Science questions

Reviewer #1: "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 Cassano et al did 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."
Reviewer #2: "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 modern circulation is to the past."

While it is true that analyzing the upper air circulation for cases such as localized cold air sinks would give additional information (e.g. when the surface becomes decoupled from the air aloft), our analysis is more focused on the large-scale synoptic situation and how that impacts thedaily weather at Lake E. Small-scale features, such as cold air pooling in local valleys, are not the focus of our analysis, and even if it was the coarseness of the model topography used in these reanalysis is insufficient to really capture this. However, the synoptic situations that do favor these small-scale situations are recognized in the analysis (e.g. a broad area of weak flow over the area of interest in cases of cold air pooling). Furthermore, we have used SLP for many previously published synoptic analyses around both the Arctic and Antarctic and obtained useful results and conclusions from that work. As seen in both the current manuscript and our previously published work, the use of SLP to define synoptic patterns is adequate to capture the synoptic

variability in local temperature, even in regions of complex terrain where cold air pooling does occur.

Reviewer#2: "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?"
This same domain and SOM analysis has been used for several study sites, largely for convenience and expense. However, the domain was chosen with Lake E in mind and the flow impacting Lake E is still well represented within the current domain size. The orientation of the isobars in the Lake E area (or lack thereof in cases of weak flow) is clearly giving us a picture of not only the localized flow, but that of the flow upstream. As such, we do not view the use of the current analysis domain as a hindrance to our analysis of synoptic impacts on the weather at Lake E. We have updated the text to reflect this.

Reviewer #1: "Where appropriate some mention of how results from the present study related to other recent work (egSerrze, Zhang, Walsh, Finnis) may be helpful to the reader. For example, what do other studies suggest about the seasonality in recent warming? The mechanisms speculated by the authors to be occurring have been discussed in other related studies."
We have added a paragraph at the end of section 5 to address this comment.

Reviewer #1: "The authors need to touch more on the important paleoclimatology issues, controls, and forcings, particularly from climate model simulations (with appropriate citations)."
The scope and content-level of this paper are already quite large, as most reviewers have noted, and we feel that our Discussion section addresses much of these concerns already. We view this paper as a starting point for more in depth comparisons with in the Lake E community, and this special issue is designed to bring together all of the best ideas into a single location so that we can pursue deeper synthesis issues. Thus far, the only paleoclimate modeling done here is being published in this special issue and the rest of the special issue touches on all of the paleoclimatology issues.

Reviewer #2: "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?"

We considered various ways to address this comment. Unfortunately we could find no clean way of doing this in the abstract without making it substantially longer. We did modify the Discussion section quite a bit, strengthening the lead paragraph about the problems with relevancy and the subsequent paragraphs on potential strengths of relevancy. But in the end, we do not have enough information to further confirm or deny relevancy. We did add the suggestion in the Discussion that should the Lake E core reconstruction be compared to other reconstructions within the SOM domain (or a larger domain), and if these comparisons are predicted by the SOM analysis, then the SOM analysis would likely be validated on their own, since the same physics underlying both the site specific and teleconnectivity weather dynamics.

3) Minor editorial suggestions

Reviewer #1:

"3- Throughout the paper, the numerous acronyms make reading very confusing. The authors should consider writing out more plain text."

We're not entirely sure what's being referred to here, but we changed NCEP/NCAR to NNR throughout the paper as another reviewer suggested and we rewrote DD as 'degree-days' throughout. The acronyms used here include SOM, PDD, NDD, DJF, MAM, JJA, and SON. We have attempted to make their use clearer, but if the editor wished we could create a table.

"3- (continued). Figure 1 is too tough to easily read. I know Cassano et al have successfully used this style in previous work, but I 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."

This figure has been modified to show lake location. We agree, there is a lot of information shown here, and we'd like to have each of the 35 plots made into its own figure for clarity, but practical constraints prevent this. We have made it clearer in the text that this figure should be at hand at all times when reading the text.

"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?"

We would like to make these sections clearer or the figures simpler, but without specific suggestions we do not see how. The figures contain a lot of information in a compact amount of space, a mean and standard deviation for 35 weather patterns, and we cannot see how to make them simpler without splitting the mean and standard deviations into separate plots, but this seems overkill. The text describes the essential scientific points of these figures, such that casual readers do not even need to refer to the figures. But in the end, as other reviewers have pointed out, to really get the most from this paper, one will have to spend some time getting used to the figures because there is a lot of information there, even though there are only two variables described.

"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."
Here the reviewer is correct that the focus of the paper is largely to explain the trends in air temperature based on weather patterns and the bulk of the text is devoted to that. However, we need to motivate the synoptic analysis based on parameters that matter to core proxies, like air temperature, and that's what these two figures are doing. We believe the abstract spells out quite clearly our conclusions about the relationships between air temperature and synoptic patterns. We have also added text to the figure captions, such that those only looking at figures will come away with more information.

"7- Section 7 rambles quite a bit and is obviously kind of different than the rest of the paper."
We have eliminated the 2nd to last paragraph, which was likely what this comment was referring too, and tightened up the last paragraph.

"8- The bibliography is very poor in regards to work not by Nolan and Cassano, and in terms of the appropriate synoptic and paleoclimatic literature."
We have added a paragraph summarizing some related work by others at the end of section 5.

Reviewer #2
"I found no location map of any sort in this manuscript... Figures 1 and 3b really need to show the lake location on every pattern.... Less significantly, misspelling of the study location name in Fig 3a is difficult to excuse."
We have not added a separate location map to save page charges because this is part of a special issue with 20 other papers on the lake, but we have added the lake location as suggested and fixed mispellings.

"Perhaps greater emphasis should be given to lines 23-26 on page 1499, in the abstract and concluding thoughts, as this provides important evidence for anthropogenic warming."
While this is a likely explanation for the warming, our research methods cannot distinguish causes of changes in thermodynamics only that they are occurring.

"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 isnt entirely clear."
The text numbers in Fig 4 sum to 365 days, as they are frequency not magnitude. The text numbers in Fig 5 do however sum to NDD and PDD. We have tried to make this clearer in the text.

"2. Fig. 8 discussion on p 1496: Please clarify the intent of lines 8-11 (eg 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 ...."

We have clarified this statement in the text. Our point was to indicate that the trends in DJF were comparatively small and not sufficient to explain the rise in MAAT over the last 20 years of record.

"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 the differences may be small, numerical convenience is inadequate justification."
We have updated the text to minimize confusion, but we determined that since the difference in numbers was so small and did not change our conclusions in any way that it was justified; this is standard practice in long-term climate studies, doing it the other way then confounds the comparisons with mean annual air temperature, which is based on calendar year. We pointed out our methods here such that someone else has the information needed to verify or refute our results.

Technical corrections:
"1. Fig 2(A) temperature scale is missing"
Fixed.

"2. Fig 7(A): use actual year on x-axis"
Fixed.

"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..."
Fixed.

"4. Be consistent throughout the paper, either NCEP/NCAR or NNR"
We have updated this in the text.

Reviewer #3
"Page 1491, lines 15-20: more detail need on when freeze and thaw seasons start/end. Is it the first occurrence of a temperature above or below 0C. Or three day average temperatures? What happens if the temperatures fluctuate around 0C for a few weeks?"
We have added some figures and text that clarify how these were calculated and what the seasons lenghts are.

"Page 1492, line 28: There is only one low pressure north of the lake. The text says 'low pressure centers north of the lake..' Make consistent"
We have clarified this in the text.

"Patterns 5 and 30 have the same number of NDD (Fig 5). What does this say given the same frequency but different standard deviation in occurrence? is the fact that they have the same NDD totals meaningful?"

We have tried to clarify this further in the text. The differences noted here are meaningful in that the same means with different standard deviations means that one of the two patterns accounts for more of the inter-annual variability than the other.

"Page 1494, line 7: It is not clear in the figure that early summer (DOY 150) got a bit cooler as suggested. Be more specific or describe another way?"
We have updated the text to make this clearer. Specifically we were referring to the last 15 years of record.

"A check should be made to see that defining negative degree days over calendar year is not causing a bias with respect to NDD calculated over the cold season."
This has been done and it does not cause a bias.

"Page 1496, line 26: What is DDJ? Should this be DJF?"
This was a typo and was corrected.

"no labels on colorbar in figure 2A"
Fixed.

"Consistency needed in use of NCEP, NCEP/NCAR, NNR"
This has been updated in the text.

"text around figure should be larger or bolder. For example in Fig 3a the text 'Temperature Anomaly" is clearer than other axis numercial labels. All text should be same boldness and generally larger"
We have tried to make all of the figures easier to read.