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Climate of the Past Discussions

Interactive comment on "Analysis of local AWS and NCEP/NCAR reanalysis data at Lake El'gygytgyn, and its implications for maintaining multi-year lake-ice covers" *by* M. Nolan

Anonymous Referee #1

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General Comments

This paper's goal is extremely ambitious: investigating lake ice dynamics over 3 million years based on 6-7 years of measurements at a single, unmaintained weather station and ∼40 years of reanalysis data; issues of temporal and spatial scaling are huge. Furthermore, undertaking an analysis based on a time when climate conditions are changing at an unprecedented rate (i.e., previous 20-30 years) adds additional complexity.

This paper is loaded with ideas and information which should be published, as any effort to help understand such a long sediment record is a valuable contribution to

knowledge of regional and global paleoenvironmental conditions. However, the scope of material provided here – and the organizational structure – renders the manuscript difficult to read and digest, especially the most-important-yet-rambling sections 5 and 6. This problem is perhaps exemplified by the final paragraph, with anecdotes about sliding on the El'gygytgyn ice surface relative to those in the Dry Valleys. Although interesting, they would be more valuable if organized better and within a discussion section, rather than as a conclusion.

In preparing this review I compiled numerous specific comments and technical corrections, yet have decided that this manuscript will require a more fundamental reorganization and/or expansion to effectively disseminate all the information and ideas. As written, the paper is difficult to read and does not provide clear, convincing results. This evaluation is not offered lightly, but after reading through and pondering the manuscript numerous times, it is the conclusion I reach.

The cursory synopsis of local AWS measurements presented in this manuscript is not an adequate foundation upon which to confidently interpret reanalysis data – especially during a time of rapid climate change. Too much of the manuscript then relies upon and discusses the implications of tentative relationships, trend analyses, and understandings. Although the scientific questions are both relevant and important, this manuscript needs additional analysis to support the extensive implications provided. In short, the paper needs a better balance between the foundation (data) and the tower of ideas built upon it. One approach to consider would be to split the paper, with one part (i.e., new paper) presenting a more careful and comprehensive analysis of the local and reanalysis data, and another discussing and speculating about ice cover.

As written, section 2 (Local AWS description and data quality) is rather strange for a paper whose title begins "Analysis of local AWS. . ." and then which continues without fully doing so. Operating a station for 8 years with little maintenance followed by such a meager analysis yields a commensurate level of confidence in the interpretation and application.

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Some statements or interpretations in the manuscript would benefit by further justification, including statistical analysis. One example is within the first paragraph of section 4, discussing seasonal variability of reanalysis temperature: "Thus it appears that in the modern environment that winter temperatures show the most variability." Yet, in the figure upon which this statement is based (Fig. 5), PDDs vary by more than a factor of 2, whereas NDDs prior to 1989 vary maybe 20% from year to year. Even over the full period, the relative variability appears no greater than for PDD.

I find no gratification is submitting a critical manuscript review, especially when (speculating here) the author is anxious to conclude this particular project. However, I think an effort to expand upon some portions and reorganize others will be in best author's and the project's best interest.

Specific Comments 1. The abstract refers to a "slight warming trend" in MAT at the AWS over the 2002-2008 period. This merits elaboration in the text and certainly needs a P-value. 2. Please provide a reference to the statement in line 22 (p. 1448), and correct the grammar: "...as is typically for these sensors." 3. In terms of air temperature and humidity measurements, why not use those from 3 m rather than 1 m? Although it appears that one less year of data is available, this would avoid several issues such as reducing the influence of near-surface temperature gradients and inversions typical over Arctic tundra, and reducing the impact of snow accumulation (e.g., measurement height = 0.5 m when 0.5 m snow). Fig. 1 illustrates problems with the 1 m temperature shield location, including heat emitted by the solar panel and reduced wind circulation. 4. Some sort of summary statistic for wind would be helpful (e.g., median daily speed), to assess radiation loading error in the sensor shields (esp. if temperature at $\sim$0.5 m (winter) to 1 m is used). 5. Why absolutely no analysis of sonic ranger data? This would seem useful as a timing and magnitude check on the tipping bucket & reanalysis measurements, and would certainly provide information about the seasonality of snowcover – something that is crucial to lake ice development and maintenance (cf. p. 1457, line 7). 6. Figure 8 provides a nice way to convey information about the

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thaw season. This would be a great place to also present or reference AWS data on temperature, snow cover and soil temperature/moisture – especially since the greatest change is since project measurements began. 7. p. 1451, line 17: Assessing the correspondence between the datasets is not easy on Fig. 5, given the short overlap. There are several more minor issues with this figure, and it took some time to figure out what the lines w/o symbols represented. I think the red line is positive degree days and the blue line negative. Mixed tick marks on the right-hand y-axis are confusing. 8. p. 1452, line 7: About winter inversions not being captured in NCEP data. This seems like a reasonable hypothesis, but then why (cf. Fig. 2) would fall and winter NCEP temperatures be so much lower than at the AWS? 9. p. 1458, line 5: I would prefer degree-days to be calculated for each cold season rather than on the basis of calendar year. Yes, differences may be small, but numerical convenience is inadequate justification. 10. Fig. 2 and others: I suggest either titling the x axis either "day of year" or use actual dates (e.g. months) rather than the incorrect term Julian Day (i.e., July 19 2003 is really Julian Day 1,728,563 – by a calendar that is no longer used).

Technical Corrections 1. Spelling of lake name in title 2. Be consistent throughout paper, either NCEP/NCAR or NNR

Interactive comment on Clim. Past Discuss., 8, 1443, 2012.