Response to Reviewers on Nolan's lake ice paper

Overview

In terms of suitability for publication, all the reviewers had the same general sentiment that there was worthwhile analysis here, but that the structure of the paper was annoying and confusing. The essence of these comments is captured here:

Reviewer #1: 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... 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.

Review #2:
As such the use of these models to examine the ice cover in the past is ambitious. Having said that, the rationale behind this paper is interesting and the author seems to be aware of some of the shortcomings of the research. The paper is organized in a way that doesn't conform to the classical format of a scientific paper. I think that reorganizing it with a defined Methods, Results, Discussion and Conclusion section would improve it substantially. At present, the paper reads as one long Discussion. This discussion meanders somewhat and lacks the rigor of a more formal scientific paper (ie, what are the objectives and rationale, what methods/approach will be used to meet these objectives, what data were produced from these methods, how can they be interpreted).

Review #3
The author is very frank about the assumptions and relative simplicity of the approach used, but ultimately provides some reasonable answers that 1) summer temperature are the key driver of melt timing, yet most recent climate warming has come in the winter, and 2) a reduction of 4 deg C from modern MAAT is what is required to cause a shift to perennial ice cover. This seems like a useful starting point for developing a long-term record of Lake E ice cover regimes and also useful information for lake core interpretation. I also think that a very nice discussion is provided about how ice growth and decay likely works on Lake E and how this might impact sedimentation rates / delivery processes and lake water levels. However, I also think that some of these very useful aspects may never be discovered due to the generally poor organization and quality of writing.

In response to these comments, I have substantially re-organized and streamlined the text to stick to the main point -- how much colder than present does it need to be to maintain a multi-year ice cover at the lake? I have also added much more field data and model calibration, and presented the paper in the more common methods, results, discussion, etc format. The paper initially started out in a different direction and I had thought it best to offer as much weather information as possible for the benefit of the Lake E community, but obviously this did not go as well as planned...

I have addressed all of the reviewer questions, comments and complaints individually below; reviewer text is in italics and my response immediately follows.

Responses to Reviewer #1

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.

I split out several parts of the AWS analysis as suggested and included it into a different paper and addressed these concerns in the new draft by adding new validation data.

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.

The section on AWS has been substantially revised and focuses on air temperature and precipitation.

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.

This statement has been rewritten, the point was not the percentage variability but the magnitude of variability.

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.

I believe that stating that the fact that the recent mean was 2 standard deviations greater than the previous mean is sufficient statistics to make the point that a change has occurred, and none of the conclusions of this paper depend on how significant this change is. In any case, I removed the statement from the abstract.

2. Please provide a reference to the statement in line 22 (p. 1448), and correct the grammar: ". . .as is typically for these sensors."
This statement has been removed.

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.
Yes, ideally the 3 m temperature is a better choice, but the fact that it has a large data gap and that the differences between sensors do not affect our conclusions warrant using the lower one.

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 ~ 0.5 m (winter) to 1 m is used).
I have removed the section on wind.

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).
I have revised the text to indicate more clearly that the best part of this record has been analyzed in a different paper.

6. Figure 8 provides a nice way to convey information about the 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.
I have revised the scope of the AWS presentation to just focus on air temperature and precipitation.

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.
I have tried to make this clearer in the text. Use of dual axes may be confusing, but it saves a separate figure and separate page charges and keeps related data within the same plot.

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?
That figure was a bit confusing because it does not show actual daily values but min max and mean. I have taken the AWS data off of it as it was not necessary there.

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.
Well, I disagree, since mean annual air temperatures are calculated on a calendar basis and all of modern science is limited by numerical inconvenience one way or another, but in any case I have revised the text.

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).
It is common usage in science to call these julian days and everyone knows that we are not counting days since the roman era.

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

Responses to Reviewer #2

General comments:
This article uses AWS and reanalysis climate data along with models to estimate what temperatures are required for Lake E. to maintain a mulit-year ice cover. The AWS and NNR data are sufficient for this purpose but the model(s) require ice thickness to work properly and this is apparently lacking. The models were calibrated with one year's data but their performance has not been evaluated.
I have included more field data for both model tuning and comparison. It's not a lot but I think it substantially improves the analysis.

Site description: As a standalone article, it would be valuable to include some rudimentary site description on Lake E. - size, depth, shape, etc as well as a map of the lake.
I added a location map.

AWS: AWS data were included to compare with NNR data. The purpose here is to show that the NNR data capture the climate of the lake adequately to drive the models. The correlation between these two data sets is not a very good measure of this. The fact that the air temperature from one method is not statistically independent from the

air temperature from another method violates a key assumption of correlation analysis (no wonder r is very high – you are confirming that the seasonal cycle of both temperature series are similar). A better measure here would be the RMSE value to show the difference between NNR and AWS. What RMSE error level would be tolerable for your purpose? Can/should the relationship between the NNR and AWS be used to estimate Lake E local temperature (ie., is the slope 0 and the offset/intercept a constant value)?
I have revised the text to strengthen support for the relationship, which is not perfect. The error due to this misfit was analyzed and incorporated into the text.

Finally, why not examine the PDD/NDD differences between the data sets in addition to or instead of the air temperature?
This is done in figure 3.

It is hoped that an 'apple to apple' comparison was made between AWS (2002-2008) and NNR (2002-2008) not to NNR (1961-2009).
This figure was misleading and I have removed the AWS data from it. There were no correlation made between means.

When using NNR to show 'modern day' conditions, why stop at 1961 when the data set goes back even further?
This was the highest quality part of the record and sufficient for our purposes, and the text has been updated.

Considerable text is devoted to the description of data availability for the AWS. While we can all appreciate the issues with gathering the data (and some of the unusual circumstances you reveal), the reason for data interruptions are not relevant to the analysis at hand. Also, letting the reader know to take care when interpreting the data and that sensors were flaky, does not instill confidence - either it is good enough or not. Focus only on the data that you need for the analysis (why describe the whole station – net radiometers etc...?) A table showing the instrument, make/model, installation height and periods of operation might accomplish this efficiently.
This section has been substantially streamlined. I disagree that describing an instrument as flaky is a bad thing, especially for people not used to working with real data, but in any case I have removed this section.

No information is given on how ice thickness was measured and in what years. This is critical for the calibration and validation of the ice thickness model (equation 1). As well, more information should be given regarding the remote sensing evidence (onset of ice on and off). This should include how ice on/off is determined and how often the imagery was acquired.
The paper has been substantially revised to include more field and remote sensing data, as well as dates and references to it.

A more rigorous approach is warranted for the model development. Both the ice growth and melt models are calibrated against 1999-2000. This appears arbitrary to me without any knowledge of the ice thickness data. There is no independent validation of

the model and an attempt to do so would lend some credence to the model hindcasts and estimation of MAAT required for multiyear ice cover. Perhaps remote sensing data of ice on/off can help? Tuning of the model is critical (by your own admission). The application of these models that are calibrated with one data point and not tested is highly suspect.
I have made it clearer in the text that we did not develop these models, we are simply tuning the empirical parameters to fit this lake. Also the validation of these choices has been expanded considerably.

To evaluate the model performance, it might be advisable to compare the critical MAAT temperature for multiyear ice formation to the MAAT at lakes with perennial or residual ice covers. The dry valley lakes is one possible comparison, but the (non-alpine) lakes closest to Lake E with perennial and residual ice are on Axel Heigberg and Ellesmere Island as well as Greenland. The MAAT in these locations is on the order of -18C. The model here implies multiyear ice at -14C.
Again, these models have been used at many other lakes. All that needs to be done here is evaluate the choice of tuning parameters, and these are site specific. Though I agree it would be neat to compare a wide variety of arctic lakes with these methods.

One of the messages of the paper seems to be that only the summer melt matters and that the growth of ice is not a determinant of multiyear ice cover. I don't dispute the importance of summer melt, but I am unconvinced that ice growth is not important. Part of this conclusion might arise from the models chosen to examine ice growth and melt. The growth model is proportional to the square root of NDD whereas the melt model is proportional to PDD. It is therefore an algebraic certainty that PDD is more important than NDD. These models are not formally tested in this paper. Also, given the recent change in winter temperatures and their variability, it seems that winter temperatures may play a larger role than is implied here.
I agree that the equations guarantee that summer is more important, but also that these equations realistically capture reality, so stating that summers are more important is not simply a quirk of the models. I have revised the text to hopefully not make it seem that only summer temperatures matter, but that multi-year ice is simply more sensitive to them.

Technical corrections:
pg-ln 1444-1 Using the first person plural makes no grammatical sense
Corrected.

1444-24 DD – pick terms/acronyms and stick with them DD (in general) and PDD/NDD
Corrected.

1445-20 – change to understanding of interannual variations in ice
Corrected.

1445-21 – the record is 3.6M years long, the core has a length in meters.
Corrected.

1446-11 - NNR or reanalysis (pick one way to refer to this data set and use it throughout)
Corrected.

1446-20 – when you say sampled, do you mean sampled and reported? When you say logged hourly – is this an average of samples taken within the hour?
Yes, it is an average of samples logged every hour.

1447-6 – what levels were the thermistors at?
This is not relevant to the paper, I point the reader to a paper that describes these data.

1447-9 – did you have glycol in the tipping bucket to melt snow?
The tipping bucket did not accumulate, it just drained.

1449-10 – I can see that undercatch would result in lower values, how can you get higher values?
This was an awkwardly phrase sentence, it meant higher undercatch.

1451-13 – 2-meter temperature. What product is this exactly (1000 mb or sigma .995?)
I'm not sure what's meant here, we used the 2m AT field from the NNR as stated in the text.

1451-13 – where is this grid point? How far away?
Well, each point represents an area roughly 200km on a side and the lake is within it.

1451-14 – compared well with... conclusion before evidence is given. (see remark about the organization of the manuscript).
Corrected.

1451-18 – please cite the figures in order
Corrected.

1451-20– Figure 2 – why do you compare 1960-2009 NNR average with 2000-2008 AWS? Please keep to the same time period when comparing. Especially considering the trend over time!
This figure was apparently misleading, the point was not to make a statistical comparison just show the data were in roughly the same range. I have removed the AWS.

1452- 3 the trends – replace with the seasonal cycle...
I meant the daily trends and have corrected this.

1452-5 – if you observed dirunal swings in temperature, then report this (there is no suggestion).
Of course there were diurnal swings in summer, but this has been revised.

1453-8 – when reporting differences, make sure it is explicit which data set has the

higher or lower values
Corrected.

1454-24 – replace actual with estimated or modeled
This section has been removed.

1454-26 –there is no fig2b
Corrected.

1455-2 – change in summer or annual PDD?
I dont understand the question.

1455-2 – why is there no corresponding trend? This is odd! If the time >0C is longer and PDD is not increasing then the temperatures must be lower (summer cooling).
This section has been removed.

1455-15-25 – important to mention snow as an insulator here, also the potential for snow ice (white ice) if the weight of the snow is enough to cause slushings (maybe not?) Any liquid precipitation represents latent heat input and this is important as well.
Corrected.

1455-26 – lake drying? Ok, need info.... How deep is it? What is the bathymetry?
I have revised this section and included relevant info.

1456-7 – and creating a need for... awkward
Corrected.

1456-23 – the units for degree days are exactly that – oCd not simply degrees
Corrected.

1457-12 – snowpack dynamics constant? This assumption is not met in the record you have, let alone over the past 3.6M years.
I disagree and have tried to clarify the text.

1457-16 – 1999-2000 why this year? Ice thickness methods?
We wrote this paper in 2001. I have tried to make this clearer in the text.

1457-17 – the lag is an interesting idea. Is this part of the model? If not, can you modify eq. 1 to include this?
Done.

1457-19 – remote sensing method – is this robust to very thin ice that can easily be confused with open water?
Yes, both SAR and landsat were very good at discriminating open water.

1458-4-9 – why can't you calculate the NDD properly? It may be a small error in the

grand scheme but it seems to be a fairly straightforward calculation.
I can and have done that in the plots. The issue is that mean annual air temperature is calculated on a calendar basis, so DD should be too. But it makes no difference to the conclusions either way, since for our primary analysis we use a mean of a single year's daily values with the implicit assumption that each year in the past is identical to this.

1458-13 – could your parameter be higher than others because of the inclusion of a lag?
No. I tested this and added more discussion in the paper. Using additional calibration data I have reduced the parameter further.

1458-17- growth is not as important as melt – can you supply a reference?
I have tried to clarify this in the text.

1459-12 – your thermistor string measures soil temperatures, how do you know the water temps?
This was a thermistor string in the water deployed in 2000. I have clarified this in the text.

1459-29- winter ice melt??
melt of the ice formed in winter.

1460-22-23 – you stated this backwards
Corrected.

1461-4 – shifted the . . . +5 to -9... this is awkward and confusing.
Corrected.

1461-5 – seams typo
Corrected.

1461-7/8 – summer drives melt – this is trivial.
Corrected.

1461-10 - -3.5C is what we believe is the minimum MAAT... - No! This is the offset to the current MAAT...
Corrected.

1461-12 – picked 20 cm – this is indeed arbitrary since it is doubtful that an ice cover that thin would survive mechanical breakup. An average thickness of 80 cm to 1 m might be more reasonable. (note that much of the ice structure would be melted internally)
This section has been substantial revised. The point to remember is that this is parameterized ice thickness, not actual ice thickness.

1461-16-18 – what about NDD?
PDD is driving the multi-year ice dynamics.

1462-19 to 1463-23 – I find the lake drying discussion tangential to the task at hand. This could be eliminated to focus the manuscript. What is the lake depth, bathymetry?
Corrected.

1465-7-9 – fine for terrestrial processes, but aquatic processes can continue in the water and ice cover despite below freezing air temps.
Agreed.

1465-10 completely eliminate summer – you mean melt-season.
Here I think its implied that I'm not talking about the calendar definition of summer, but I have rewritten this.

Section 6 (relevance to paleo...) could be tightened up and focused.
Done.

Responses to Reviwer #3

General comments
Additionally, many basic details about the site characteristic and datasets used are neglect or simply referenced from other work, making this a difficult paper to review without additional research.
I have added more relevant details.

Specific comments:
P1443. Title: suggest not using abbreviations in title and simply saying "Analysis of local weather station and reanalysis data. . .". I understand that these are fairly well recognized terms and that NCEP/NCAR is used in citation for this model product, however I think a title should be able to stand alone. Similarly the acronym for NCEP/NCAR is never stated in manuscript, nor is it this model product described at all in methods or elsewhere. It is commonly used, but some description needs to be included.
Corrected.

For AWS, aren't all weather stations AWS now, so just saying weather or meteorological stations seems better.
They are not all automatic, but AWS is simply a shorter description too.

P1446 – This is a well studied system, but still some site description is warranted including a site map and location of the met station and domain of reanalysis grid cell used relative to lake and station.
Corrected.

P1445-1451 – For description of the weather station and its operation, I'm not sure this level of detail is necessary, particularly the exact circumstances for early system malfunction. Data from many of the sensors described here weren't used at all in the analysis (i.e. snow depth, soil temp) and thus don't seem necessary to include. Though

analysis of snow accumulation and melt, even if not representative of the lake, could have been useful in ice growth modeling and of general interest to hydroclimatic conditions. Additionally, why not call the "tipping bucket" a rain gain and specify that it operates by at tipping bucket mechanism. Similar point for sonic ranger.
This section has been substantially streamlined. Tipping bucket and sonic ranger are pretty commonly used terms for these sensors.

P1458-1459 – Discussion here of ice decay process is very intriguing and relevant to paper. What constitutes full ice-out and how does partial ice-decay (development of motes and leads, but with large pans) impact subsequent ice growth and more importantly lake sedimentation rates, mixing, and productivity? This could be expanded upon it seems could be analyzed using re- mote sensing products (i.e. MODIS, SAR, landsat, etc). This was done in Nolan et al 2003 very nicely and doing such analysis that corresponds to period with local met data would seem very helpful to this story to verify modeled ice-out and ice-formation, but it doesn't appear to be attempted.
I did not add any new remote sensing, but I tied in our prior research much more tightly. I agree that adding another 10 years of remote sensing data would both be interesting and useful to this paper.

P1459&1463 – I assume there hasn't been any lake level reconstruction attempted here or it would be cited and discussed. Having a lake bathymetry map would be helpful to facilitate discussion in this section.
No it has not been attempted by me, though the Federov paper in this issue touches on it. I presented a bathymetric map in the 2007 paper.

P1464 L3-6 – It would be nice if provide at least some details here as to synoptic drivers of air temperature, but instead have to look at companion paper.
I've tried to streamline the paper rather than make it ramble more, so readers are stuck having to read the other paper.

P1466-1467 L28-6 – I'm not sure I follow the logic in how seasonal vs. perennial ice cover would impact sediment transport and deposition. It seems only on years with initial congelation ice formation with snow free period, would lake ice be bare. And if these conditions did occur, they could promote substantial sediment transport across the smooth surface which wouldn't occur with a snowpack and this could be a mechanism for increased sediment deposition into the main body of the lake that is of interest.
I have removed this section.

Table 1 and Figure 4 and Figure 9 – Why so much emphasis placed on comparing rainfall between local weather and reanalysis data? Is this with the goal of understanding lake level variability and sediment delivery? It doesn't seem relevant to lake ice growth or decay. Even if this is and I'm not understanding the objectives of this paper, why are they presented in this order?
Yes, the paper was confusing. I have tried to streamline it to address these comments.

Figure 3 – Why show daily RH here over 7 year period?

This figure has been removed.

Figure 5 – Symbols can’t be recognized and difference colors not indicated.
I'm not sure what the issue is here, it seems fine on my copy.