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Interactive comment on "Climate change and Northern Hemisphere lake and river ice phenology" *by* Andrew M. W. Newton and Donal Mullan

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Comments from reviewer John J. Magnuson Aug 28, 2020.

General comments: The paper provides a detailed analysis of a larger group of lakes and streams over 74 years from 1963 to 2005. Data are from the collection of data in the Snow and Ice Data Center supplemented largely with Swedish, Finnish data. The analysis of slopes were over 4 time periods, 1931-1960, 1961-1990, 1991-2005, and 1931-2005. There are a number of new findings that included comparisons of lakes vs streams, changes in open water duration, differences among regions, and differences among the year subgroupings. I have organized my comments below in terms of what

I liked and what I have concerns about.

What I liked.

Was a Northern Hemisphere analysis rather than a locally constrained analysis. The regional comparisons are useful. Made an honest attempt to include all of the data unless there were too many missing values. Good idea.

Used fixed time periods with less than 10% missing years. Good idea. Other researchers have occasionally had difficulties using these data because they did not constrain the time periods and therefore mixed the influences of longer term changes over time.

Analyzed rivers as well as lakes. Good idea.

Looked at the length of the open water duration. Good idea. Most limnologists have looked at the duration of ice cover over a winter season, rather than the duration of the open water over an entire open water season. Might have been interesting to think about how that changes one's perspective. Both ice cover and open water seasons play a role in the biogeochemical cycling and other ecological phenomena as related to ice cover. Is the change in one more important ecologically than the change in the other? Might have been useful to show relation over time of both ice cover and open water. Even though they relate to two sets of years, they should be highly correlated

Consideration of heterogeneity among lakes, time periods, and regions as well differences in variability. Good idea. I think they might have considered making the comparisons in regard to a set of issues, questions, or major or in respect from what they know from the ice cover literature. "Looking in some detail at regional differences" was a good idea such as northern and southern Sweden, or Europe and North America. But I am not convinced that I agree with their explanations.

I liked the quick reference list with findings from each published paper they found. Good idea. However, the results from these papers were not integrated or their findings or compared with this paper's findings. Findings stated as new were already found in the various published analyses.

What concerned me.

1. Did not indicate what was actually a new finding. It was all new, perhaps because it included all of the usable data between 1963 and 2005. But items in their final list of findings have often been reported as general findings by earlier researchers using various time periods or regions. I would have appreciated their integrating what they found with earlier findings. Such interpretations were usually absent except for a few general statements in the introduction that was an incomplete citation history. Many of the papers were in the list of papers and major results, but they were not integrated into a discussion of their own findings. How do your results compare with Benson et al 2012 using a smaller number of lakes but with longer time series; she looked at 150 year trends, 100 year trends and 30 year trends using largely the same data source that the authors used. So, "what was new", was that they used the most robust data set that existed on inland-water ice phenology. I thought the most defensible grouping of years was of the full duration of years for the waters they analyzed. The least defensible was the short time series in the most recent years. Perhaps I missed it, but I would have enjoyed reading more about the rationales for the year subgroupings they decided to analyze. Why not use three equal year groupings of the same length. How does the number of years in a set influence the results?

2. Long term. They referred to the time series analyzed as long term, but were they long enough? Unfortunately, they did not cite an important paper by R. Wynne (2000) that first revealed that running slopes of the lake ice time series alternated from positive and negative when analyzed using consecutive moving windows. Wynne looked at 4 lakes with 100 year time series of ice breakup (2 in North America and 2 in Europe). The running means both of 20 and 50 years across the time series oscillated over the 100 years between positive and negative slopes. Whether one observed a positive or a negative slope depended on the start date of the subset. See (Wynne, R. H. 2000.

Statistical modeling of lake ice phenology: Issues and implications. Verhandlungen des Internationalen Verein Limnologie 27:2820–282).

3. Oscillatory dynamics. Papers on the oscillatory dynamics of longer term data reveal a number of interacting oscillations. (e.g. Sharma, Sapna, and John J. Magnuson. 2014. Oscillatory dynamics do not mask linear trends in the timing of ice breakup for Northern Hemisphere lakes from 1855 to 2004. Climatic Change 124:835-847.) Again, the slope of subsets selected would depend on the start date of the subset. Take a look at Sapna's Figure 5 panel D and consider the result of having started a 30-year period in 1948 versus 1961 or 1960. Some of the most significant oscillations in the Sharma paper were in the range of El Nino that could easily mess up the interpretations of the shortest time period the authors used for recent years. Their most recent date group was short enough that an El Nino near the end or the beginning of that short series could be relevant. Interpreting it as a more general long-term change is problematic. Many published papers have analyzed these oscillations and the authors should have at least discussed the issue and how that might have influenced their conclusions. They cited most of them in their listing. I think it would have been interesting to do an analysis like Sapna's over the 74 years for different regions. A simple set of running means as in her figure 5 might be sufficient. Then compare those rather than for the three periods the author's used (two of equal length and one short).

4. A point on the large-scale climate drivers like NAO or AO. The Livingstone (2000) paper they originally credited for the roll of these kinds of drivers on ice phenology found relations not only with NAO but also with the SOI. Lake Mendota, for example, was influence by both. I think the authors should have included SOI in their analyses. (Livingstone, D.M., 2000. Large-scale climatic forcing detected in historical observations of lake ice breakup. Verhandlungen des Internationalen Verein Limnologie 27:2775–2783.). Other papers the author's cited also found that SOI was also an important correlate.

5. What other papers pointed out that ice cover was related to air temperature and

the climatic variables. A discussion that included how these results compare with the author's findings would have been interesting to me.

6. The comparisons they made with lakes and streams were new and I liked that. The only other paper to have done that was the Magnuson et al. 2000 paper in Science. Did Magnuson et al. see a difference between lakes and streams? If not, this is a new finding. I thought that the authors causal explanation for the difference between lakes and flowing waters could be expanded as it did not include some of the most important processes. I think additional factors such as the greater depths of most lakes and the thermal stratification of lakes both should play a role. Rivers tend to mix over the entire water column, only shallow lakes do.

7. I would like to see more explanations of the results based on the authors findings and the literature on the inland water cryosphere. A more in depth explanation of why freeze dates are less directly related of air temperature than are breakup dates, for example. There are brief interpretations of that observation in many of the papers they cite. The literature on the various mechanisms generally are not cited but are known.

8. The paper was a bit exhausting owing to the detail and number of comparisons. It would have been good either to identify major issues rather than describing all of the detailed results or to focus the structure on the regional comparisons. This ended up being a description of what was happening everywhere in great detail. To some extent this is basically a detailed descriptive data report.

9. The authors frequently tried to explain differences among individual lakes rather superficially, eg. Mystery vs Nebish, or Mendota vs Monona. May not have the local knowledge to work at that fine individual lake scale. I would put the analyses into the broader result rather pick apart individual lakes. particular lake

Other suggestions: 1. I would give the years of observation in the title, i.e. 1931-2005.

Minor points.

In the maps take a look at whether the shades of grey are distinguishable from each other or the background. I had difficulties doing so.

Madeline Island and Bayfield data are essentially identical and rightly so. If it is the data set with which I am familiar it comes from the periods of ice cover that excludes boats and ferries from entering or leaving Bayfield. The ferry runs from Bayfield to Madeline island in open water seasons and an ice road is used in winter between the two locations. The early paper on these data is: (Howk, F. Changes in Lake Superior ice cover at Bayfield, Wisconsin. J. Great Lakes Res 35, 159–162 (2009)). There is an additional Chequamegon data set farther up the Bay.

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