The Cryosphere Discussions

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Comment on tc-2021-52

Anonymous Referee #2

Referee comment on "Recent changes in pan-Arctic sea ice, lake ice, and snow-on/off timing" by Alicia A. Dauginis and Laura C. Brown, The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-52-RC2, 2021

Review of "Recent changes in Pan-Arctic sea ice, lake ice, and snow on/off timing" by Dauginis and Brown

Summary

This paper presents trends in the time of sea ice, snow, and lake ice presence based on the IMS product. Correlations are done with ERA5 2 m temperature data. The trends show earlier sea ice retreat and later freeze-up, in agreement with previous studies. The snow trends indicate earlier snow melt but also earlier first snow, so that trends in snow length are small. There is regional variability with the Bering and Chukchi Seas showing the greatest warming.

General Comment:

Overall, this is a competent manuscript. The data used are generally good (though see some comments below on this aspect) and the analysis is solid. However, it is lacking a coherent "story". The goal of the paper is to simultaneously highlight changes in sea ice, lake ice, and snow, but each is largely treated in parallel – as if there are three different papers just merged together. I'd like to see more discussion of connections and interactions, covariance in parameters, etc. For example, Figure 6 shows snow/ice-off trends and I see a distinct difference in Lake Ladoga versus the surrounding snow on land with the lake showing a much stronger trend toward earlier onset. I don't see a similar pattern in the Canadian lakes for snow/ice-off, but in Figure 7, some lakes – e.g., Great Bear Lake - do show a unique trend compared to the surrounding land. Digging into these types of things would be pretty interesting, but they aren't discussed in the paper. In short, based on the title and the abstract, I was expecting more of a co-variance analysis – how are lakes and nearby snow on land and sea ice correlated and how might the forcing mechanisms affect them similarly or differently.

The correlation with temperature is also rather superficial, simply noting correlation coefficients. I don't think the paper needs to be an in-depth attribution study. But simply noting correlation doesn't really provide much insight. I note below a couple examples where the paper could dig in a bit more – e.g., what is the cause of the correlations? The way it is presented, it suggests that higher temperatures result in earlier ice/snow loss and later ice/snow formation, but it's not necessarily that simple. For one thing obviously, there is more than just SAT going on – there are winds, SLP, ocean temperatures, sea ice advection. Just looking at SAT may not tell one much, especially in specific regions.

I also think that Section 3 is very wordy and somewhat hard to follow – it is one number after another. The data are all in the figures and tables, right? So, I don't think you need to worry so much about putting the numbers in the text. Focus on describing the main characteristics – significantly positive and negative trends, regional variations, etc. I also found the mixing of trends and correlation a little odd. I can see the rationale to relate those in the discussion. However, you are showing trends, but then correlations with de-trended data. So, it feels a little inconsistent, though I understand you're trying to relate temperatures to the snow/ice changes. You might consider splitting the two into separate sections: first the trends, then the correlations and use the correlation to discuss the relationship between temperature and snow/ice on-off dates. Maybe that won't work well, but something to consider trying.

Finally, more discussion of the data is needed (including proper citation as noted in the comments below), particularly data quality. IMS is described reasonably, but because IMS is an analyzed product and has used differing amounts and quality of data over the years, one should use caution when applying it towards trend analysis. I think it generally is okay in this context, looking at overall trends, but I think a comparison (at least for snow and sea ice) with passive microwave products would be beneficial to demonstrate consistency; at the very least, a discussion of the limitations of the dataset is needed.

This goes doubly so for the temperature data. There needs to be more than a simple two-sentence paragraph. For one thing, reanalysis products are notorious for issues in polar regions because the data sparsity, particularly over sea ice. I note in comments below a couple artifacts that jumped out at me in the figures. And 2 m temperatures can be particularly problematic because the reanalysis models don't necessarily capture the surface boundary layer accurately. A now somewhat outdated paper (Lindsay et al., J. Climate, 2014) discusses some of the limitations and biases of different reanalysis products in the Arctic. There is nothing wrong with using ERA5 – it is one of the more updated and better products – but I think it is important to discussion the uncertainties and limitations. Along these lines, it might be interesting to correlation with 925 mb temperatures in addition to SAT (2 m) fields as that gets above the boundary layer and the reanalyses may be more reliable/consistent there.

There are several fairly major comments here, along with the specific comments below. Thus, I recommend major revisions. However, I think the comments can be addressed with some thought and some rewriting and additional discussion, which I don't think should be too onerous. So, I think these revisions can be done in a reasonable amount of time.

Specific Comments (by line number):

56: I understand there are a lot of ice melt/freeze papers, and there is no need to be comprehensive, but it might be best to focus on more recent papers. E.g., Markus et al. (2009) has been updated by Stroeve et al., GRL, 2014, and even more recently by Bliss et al., Remote Sensing, 2017, and Bliss et al. Env. Res. Letters, 2019.

102-109: I find the region definitions somewhat arbitrary and each encompasses quite different conditions. The Canadian Arctic spans the CAA, which has landfast and MYI, along with Baffin and Hudson Bays, which are nearly all FYI; and then it extends to a seemingly arbitrary longitude, east of the Canadian/Alaska border – so it not all of the Canadian Arctic, but most of it. The Alaska/Far East similar includes the Chukchi and some of the Beaufort, which have MYI and FYI, plus the Bering, which is exclusively FYI. Eurasia, seems like a catch-all region for everything else. And looking at ice/snow on-off dates is going to be at least partly latitude dependent, so splitting into regions with a wide range of latitude within them would seem to muddle any relationships. I agree this simplifies the analysis having only three regions, but I think explaining better the rationale would be helpful.

And similarly, is there a rational for using 56 N latitude as the southern boundary? It also seems somewhat arbitrary – it cuts off the southern Hudson Bay, for example. I guess it maybe encompasses the southern boundary of ice within the Bering, and it includes the large Arctic lakes. Thus, I can see a potential rationale (if I'm correct), but it should be explained within the text.

111-114: Make sure you are clear on the source for the data. You mention NIC, but it has only recent data I believe. The historical archive is at NSIDC: https://nsidc.org/data/G02156/versions/1. This is referenced a few lines later for more information, but I think it is important to be clear upfront that the data were obtained from NSIDC (assuming they were). Also, the appropriate citation should be provided for the data set, as noted on the NSIDC page:

U.S. National Ice Center. 2008, updated daily. *IMS Daily Northern Hemisphere Snow and Ice Analysis at 1 km, 4 km, and 24 km Resolutions, Version 1*. [Indicate subset used]. Boulder, Colorado USA. NSIDC: National Snow and Ice Data Center. doi: https://doi.org/10.7265/N52R3PMC. [Date Accessed].

114: The NIC web address has recently changed, so it needs to be updated to: https://usicecenter.gov, or more specifically: https://usicecenter.gov/Products/ImsHome

123: Similarly, for temperature data, more specificity is needed. Is there a website from where the data were obtained? And, if available, a full citation should be provided or at least the data set DOI (if one exists).

128: Maybe not directly relevant here, but could be worth mentioning that there is now a 1 km product starting in 2014. I wouldn't expect these to be used in the study as the time frame is too short to be of use, but readers may be interested to know that.

145: Were the data re-gridded/re-projected onto a consistent grid for comparison?

198: Here is an example of where the paper could go farther. The authors note that freeze onset shows signification correlations with October air temperature. That's not surprising and it is not necessarily due to warmer air temperatures delaying freeze-up. As other studies have noted, it's actually the reverse: warm SSTs in the ice-free region give heat to the atmosphere and warm the atmosphere. So, it is the lack of sea ice that causes the October air temperatures.

217-220: Not discussed here is the possible role of ice dynamics. How ice moves into and through the CAA can have a significant impact on the presence or lack of ice in the region during break-up and particularly freeze-up.

343-347: Another example where more analysis/discussion is needed. First snow-on dates are positively correlated with August, September, and October temperatures. But as noted above, the Aug, Sep, Oct temperatures may be the result of lack of sea ice causing the warming temperatures. And the lack of sea ice also provides a moisture source for earlier potential first snow. There is nothing groundbreaking in my analysis here, but it is not acknowledged at all in the manuscript.

Figure 1: Not to be too pedantic, but Greenland is shaded as Eurasia, but it is technically part of the North American plate. Just changing the color key to "Eurasia and Greenland" is reasonable, though it might look better shade Greenland with the North America color, or consider a separate shade altogether.

Figure 3: This is pretty obvious, but for total clarity, it would be worth adding a note in the caption that white indicates locations where there is no snow/ice on date (either because snow/ice never occurs there or snow/ice is present all year – e.g., GrIS and multi-year sea ice). Also, in the color legend, it would be helpful to label the center date (e.g., the pale yellow color). For snow/ice on date, it is okay because the middle is near day 365; but for snow/ice off labeling the yellow would be helpful for discerning colors between the extremes of 40 (red) and 275 (blue).

Figure 8: While listed in the captions, it would be more helpful to label each map with the month (or appropriate abbreviation, e.g., "Jan", "Feb", etc.), either in addition to or instead of the figure letter. This makes it easier to read the figure – I can just look at "Sep" instead of looking at "i)" and having to connect it to the month in the caption text.

Figure 8j: This demonstrates some of my skepticism of SAT trends over sea ice. The "bullseye" of blue in the middle of the Arctic Ocean doesn't look realistic to me – probably an artifact of some interpolation (November shows a similar, albeit larger and more dispersed.

Figure captions: Just a general comment on style – putting the figure letter - (a), (b), (c), etc. – at the end of the corresponding description is somewhat confusing and makes it more difficult to read. I think it would be easier to read if the letter is placed before the description for that letter.