Major Concerns

This study looks at relationships between sea ice, snow cover and lake ice timings of retreat/advance from 1997 - 2019 (shorter time-period for lake ice). This is done on a pan-Arctic scale first and then more regional discussions follow. While some relationships between exist based on large-scale warming, earlier snow cover may be expected because of increased atmospheric water vapor from more open water in autumn. Studies have documented that the Arctic atmosphere is now warmer and wetter (i.e. Boisvert and Stroeve, 2015; Serreze et al. 2012), and links with earlier snow on land have been reported (i.e. Ghatak et al. 2012). However, none of these studies are referenced here in the discussion of these linkages, and all relationships are really just discussed in terms of air temperature, which is overly simplistic. Drivers for example for earlier melt onset over sea ice is largely driven by warm/moist air advection into the Arctic (i.e. see papers by Kapsch, Francis, Mortin), especially on the Eurasian side of the Arctic. And thus downwelling longwave has been found to be the primary driver. And for freeze-up, Steele et al. talked about the importance of mixed layer ocean heat driving the freeze-up and of course many papers have then discussed this heat release to the atmosphere driving the warming air temperatures (i.e. papers by Serreze et al., Screen et al., etc). Thus the maps, while interesting, are analyzed in terms of their inter-relationships only with correlations and air temperature. You may not be aware of a publication by Crawford et al. (2018: JGR-atmosphere) who looked at the relationship between snow retreat and sea ice retreat. The only region with a statistically significant relationship was found in the Laptev Sea with the early retreat of snow over the West Siberian Plain. They discussed the atmospheric mechanisms by which these regions could be linked. Such an analysis could be applied here in a broader context to really understand how these components, sea ice, lake ice and snow cover are interconnected. Just because you have a correlation between two variables doesn't mean you've understood the drivers of the linkages.

So then I have to ask what are we gaining from this study? The conclusions state that there are trends towards a longer snow and ice-free season, which isn't new knowledge, right? But it is interesting that the ice-free or snow-free season is largest for the sea ice, followed by the lake ice and then the snow cover. So what are the implications of that? And why would sea ice have a longer ice-free season? These are some of the questions that should be answered by this study. Otherwise it just feels like a missed opportunity.

Other Comments

As far as I know, IMS data have never been recommended for use in long-term change studies because of the dependence on analysts interpreting if there is ice or snow there or not. Analysts change and there is always a subjective element to this mapping. How are you accounting for error in your assessment? I would think at minimum checking your results at least for sea ice and snow cover against automated data products would be worthwhile. And some discussion on how your ice/snow on/off dates relate to earlier studies is needed. I realize that your time-period will differ from other studies, I think it's good to discuss a bit more about your methods for first date of no ice and date when the ice is gone for good until it comes back. Obviously this is a bit problematic for sea ice which is in constant motion. Stammerjohn et al. 2012 and Stroeve et al. 2016 had different ways of computing the retreat/advance of sea ice. This will also play into your determination of the length of the open water period. And of course this influences your trends shown for first ice-off and continuous ice off dates, and why they differ so much between first open water/continuous open water and first freeze/continuous freeze.

It is not particularly novel to discuss sea ice retreat/advance or snow off/on as this has been done already in other studies. However, I do like seeing Figures 3 -5 as it's nice to see the land and ocean at the same time. However, talking about mean values for the Arctic as a whole is really meaningless and you rightly point out that there are large regional differences. Best to focus on lags between the ocean and land regions on a regional basis. Thus, in general I think the focus really should be on the regional relationships but then some sort of advanced clustering analysis would be beneficial to first identity the regions with the strongest relationships. Crawford et al. (2018) did show some ways to do this analysis that could be useful here.

Line 45: I find this statement to be a bit strange since timing of melt onset, and melt onset trends are latitudinal dependent, so I would expect the CAA to have weaker trends in melt onset and freeze-up. For the most up-to-date trends in melt onst/freeze-up you could reference Stroeve and Notz, 2018.

Line 66: on the other hand, once there is liquid water in the snow and/or melt water, passive microwave algorithms underestimate sea ice area, so are you just referring to coastal areas here? Yes there is coastal contamination, resulting in false ice concentrations near the coast, yet in summer you also have the opposing effect of melt water.

Line 181: I do not understand your statement that sea ice off dates are most strongly correlated with temperature in September. I assume you're speaking of air temperature so you should specify this. Yet it is well known that air temperature does not drive melt onset or ice retreat (i.e. Mortin et al., 2014; Kapsch et al.). Basically what drives earlier melt onset (which is correlated with ice retreat – Stroeve et al., 2016) is advection of warm moist air masses into the Arctic and the downwelling longwave and sensible/latent heat fluxes associated with those air masses. This is especially true in the eastern Arctic (i.e. Barents and Kara seas). Maybe in the CAA air temperatures are important but this is not true everywhere in the Arctic. Also, why would you expect to have the strongest correlation with September temperatures? Unless it's a feedback that earlier retreat of open water leads to warmer ocean mixed layer temperatures that then of course help keep the atmospheric temperatures warmer. Nevertheless, I think your analysis here is too simplistic and doesn't add anything unless you look at all drivers that influence ice/snow retreat/advance. Thus, the same concerns will apply later on lines 196:200 since if you have earlier retreat of ice you will have later ice advance (i.e. Stroeve et al., 2016; Steele et al. 2016), and of course the release of the mixed layer heat back to the atmosphere will be responsible for the correlation you see (see also papers about Arctic Amplification by Serreze et al. 2009; Screen et al.)

Line 200: I believe your results are consistent with several studies indicating earlier snow fall in autumn in part because of the sea ice loss. This paper comes to mind (Ghatak et al. 2010 – JGR) but I believe Judah Cohen has also written on this.

Line 233: How is the clustering done? This wasn't discussed in the methodology section.

Lines 423-424: I disagree that delays in freeze-up are consistent with delayed snow onset over land and delayed freeze-onset on lakes. Yes, the lack of sea ice may result in locally warmer air temperatures that influence snow and lake ice formation, yet the lack of sea ice may also lead to earlier autumn snow accumulation. I feel your simple statistical analysis does not really explain the processes for the relationships observed.