Review of *Weather and Climate Dynamics* manuscript #wcd-2021-41

Winter thermodynamic vertical structure in the Arctic atmosphere linked to large scale circulation

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Overview

This manuscript describes an analysis of Arctic winter atmospheric properties associated with various characteristic large-scale circulation regimes identified using a clustering tool called self-organizing maps. The authors use reanalysis output, station data, and radiosonde profiles to investigate differences in vertical temperature and moisture profiles, cloud amount and liquid water content, inversion strength and height, and surface energy fluxes associated with varying circulation and advection regimes based on surface pressure. The manuscript is well written, figures are clear, and the analysis is thorough, but the results and conclusions mostly confirm what is already known and do not provide substantial new understanding. In general, they find that conditions in which a warm, moist flow from open-ocean areas penetrates the Arctic are associated with increased cloudiness, increased liquid water content, increased downwelling longwave radiation, and weaker inversion strengths. Circulation regimes that cause flows from ice-covered or land areas tend to be drier with lower cloud amounts, stronger inversions, and smaller downwelling longwave fluxes. The only surprising finding to me was the higher altitude of maximum temperature and specific humidity values in conditions of stronger inversions; I would have expected the opposite. Nevertheless, the study provides a very instructive summary of a complex environment that would likely be useful for educational purposes and for ongoing studies to understand processes associated with the rapid changes that are underway in the Arctic. For these applications, I would support publication. The study would have been much more interesting and enlightening if the authors had explored differences in atmosphere/regime linkages during the "cold Arctic" period (say, 1979-1996) versus the recent decades presented in this manuscript (2009-2018).

Specific comments

1. Section 4.2: It's unclear whether Fig. 2 is based on all days in the data set or a subset.
2. In Figure S2, why is the high fraction of liquid water collocated with high pressure in types 1, 3, and 10 but much less so in type 12?
3. Line 224: change shortly to briefly
4. 203-204: In my experience, using anomaly fields to create the SOM eliminate the problem of all types exhibiting some of the same features. Anomaly fields accentuate differences among types and often assist in interpreting other fields mapped to the SOM. It would be interesting to see if results changed if anomalies had been used to create the SOM, and how different the types would look compared to Fig. S1.
5. 224: change shortly to briefly
6. 268-273: Is ERA5 able to capture realistic differences in cloud phase? This seems an important question given the sensitivity of surface fluxes to this variable.

7. Section 4.3: I'm surprised that more attention has not been given to precipitable water (total column water vapor), as it has a large impact on downwelling longwave fluxes in winter, especially when clouds are absent.
8. 296: This statement may not hold as the sea ice becomes much thinner and broken.
9. Figures 4 e-p: It's very difficult to see important differences among types. I suggest instead plotting differences from median values to make differences more conspicuous. The color scale used for m-p is also not optimal for displaying differences.
10. 322: I suggest adding "and resulting inversion strength" after "by the surface".
11. 323-325: How accurate are the elevations of T and q maxima? Is the difference between the heights statistically significant?
12. 325-327: I suggest providing the standard deviation to give a sense of variability in these values.
13. 332-335: It's surprising that ERA5 and raobs differ given that the raobs are assimilated into the reanalysis. Perhaps type 1 collects a relatively wide variety of circulation patterns, which is typical for corner nodes of the SOM.
14. 360-361: Can you offer an explanation for this finding?
15. 370: "does not have any unambiguous impacts" is a convoluted statement – can you reword to clarify?
16. 412: How are temperature gradients defined/measured?
17. 419: change largely to greatly
18. 435: Please explain why a different set of SOM types from the rest of the analysis are used for this location.
19. 440-441: Could this difference just be due to the different types selected for analysis?
20. 470-471: Perhaps the duration of the high pressure is also a factor.