Review notes for the revised manuscript to ACP: "Wintertime Extreme Warming Events in the High Arctic: Characteristics, Drivers, Trends, and the Role of Atmospheric Rivers" by Ma et al.

I highly appreciate the effort of the authors to address both mine and the other reviewer's concerns and feedback, especially regarding the warm event definition and clearly stating which events are discussed in each section of the manuscript. I am pleased to see that some of our small suggestions were also adapted immediately in the revised manuscript. Thus, I find that the manuscript has now improved a lot. After addressing a few questions and suggestions presented below in terms of a minor review, I am happy to accept this manuscript for publication to the journal. The line references are wrt the reviewed manuscript.

Specific Comments

- Despite my concerns on the event definition as a grid-point defined event, the authors have succeeded to explain the pros and cons on their selected method. It is true, that grid-point defined warming events favour that one can understand more about what processes affect locally a specific grid point. Thereby, the spatial heterogeneity of characteristics, such as event duration, will be revealed, which might not be the case when looking at events over larger areas. This is also true when investigating e.g., why some grid-points experience earlier melt or freeze-up onset compared to other grid-points, something that could be missing if enlarging the study area. And I agree when the authors write that the large-scale setting differs relative to the relative location of the grid-point that experience warming. The relation to large-scale patterns can still be obtained if using area-based event definitions, but might not indicate as clearly the importance of the relative location. Anyways, thanks for all additional discussions and clarifications regarding the event definition, this will be helpful for the future readers. However, I have a short comment on the discussion authors provide on the temporal requirement between events (which is not obtained in the current analysis), and my concerns of double-counting of similar events. Figure 6 shows the temporal evolution in lag composites for your grid-point events where no interval requirement between consecutive events is applied. In the response documents, the authors provided a similar figure (R9) with a 5-day interval requirement and indicated that the composites do look alike. I don't disagree with the authors, but would like to draw attention to the long-lived events (R9g) with notable differences between the original figure wrt the turbulent heat fluxes (negative values instead of around zero) and IWV around lags -5 to -2. This would indeed suggest on somewhat double counting of events in Fig 6: are these differences only a result of less events included in R9g, or is this really a result of that the previous long-lasting event was actually part of the begin of the next long-lasting event? Maybe one additional notation of this possibility would be good in the manuscript, where Fig 6 is discussed and/or where the authors motivate reasons why not imposing the temporal constraint (around L122).
- It is nice to see the additional motivations in dividing your events into poleward and equatorward of ~85N based on the surface types. I was wondering whether the trends in any characteristics shown in Fig. 15 (thanks for the additional subplots for the concurrent events!) take into account this division (into "surface type" based on spatial restrictions)? Would the trends look different in the sub-categories?
- Abstract: I would suggest adding "2-meter" temperature (L12) as the temperature variable.
- New final paragraph: Thanks for adding this at the end to summarize and indicate possible further research topics. However, there are indeed some previous studies that show e.g., the relationship between persistent atmospheric circulation in March and the minimum sea ice extent in September (see e.g., Kapsch et al. 2019: https://doi.org/10.1007/s00382-018-4279-z), and the preconditioning of warm winter/delayed freeze-up in autumn for thinner ice next spring (e.g., Stroeve et al. 2018: https://doi.org/10.5194/tc-12-1791-2018). In

comparison to these papers, a good addition would be to study these links in the light of your identified warming events, as the authors nicely point out.

- The ECMWF is written out in two sections close to each other (at the end of Introduction and in the beginning of Methods). I would include it when it is mentioned the first time, and just use the acronym when it is mentioned the second time.
- L284: The location of the negative SLP anomalies to the southeast of the events in long-lasting events could also hint for a NAO+, a circulation pattern that would favour moisture and heat transport across the Atlantic towards the Scandinavia. The blocking-like persistent positive SLP anomalies over Scandinavia/Urals would then deflect that airflow northwards, towards the warm events. Studies also show that the decay of a NAO+ pattern could lead to an enhancement of the blocking to the east of the NAO pattern, and a warm anomaly in the Arctic. As your Fig. 9 is not exactly a geographic map (as you have centred the plots around the event location), these patterns might not be exactly over Iceland (negative SLP) and Eurasia (positive SLP), but would cover about the correct regions given that the possible locations for the extreme warm events are spatially constrained in the Atlantic sector. Have the authors thought about these possible connections wrt the long-lasting events? Some references, e.g., Luo et al. 2016 (https://doi.org/10.1175/JCLI-D-15-0612.1), Luo et al. 2017 (https://doi.org/10.1088/1748-9326/aa69d0), and Murto et al. 2022 (https://doi.org/10.5194/wcd-3-21-2022).
- L379: deep penetration of ARs associated with an SLP center located more polewards. Could this be related to locally formed Arctic cyclones, with cyclogenesis in the Arctic north of Greenland? There are studies, such as Messori et al. 2018 and Murto et al. 2022, that also find these local cyclones (associated with negative SLP anomalies polewards) to occur around the time of the warm events. Maybe worth to discuss this possibility also in the current manuscript?

Technical corrections

- Fig1 caption: I would add “in (b)” between “The purple line” and “denotes…”.
- Fig1a: to help the readers to look at Fig1a and following the first sentence in the results (at L163), I would add a thin (maybe dashed?) contour to highlight the -20°C isotherm.
- Fig10: It would help the reader to look at the figure if columns had their own titles, e.g., “all concurrent events”, “Cluster 1: strong SLP anomaly dipole”, “Cluster 2: blocking-like surface anticyclone“ and “Cluster 3: strong Greenland SLP anomaly”. Similarly for Fig. 11. The number of events in each of the 3 clusters would also be good to know, maybe add in the figure caption for Fig. 10?
- Fig 15: some lines cover the text in the legends, which makes it hard to read the legend.
- All map figures: As the latitude band of 83N is of importance in this study, I would suggest to explicitly mark that latitude in all maps.
- S2: Thank you for adding the climatological sea ice edge contour. Why did the authors choose a SIC of 50 %? As far as I know, the sea ice edge is usually marked as SIC of 15 %. If the figure stays similar with a SIC of 15 %, I would suggest changing.
- L72: “This event *was* driven by an …” instead of “is”.
- L91: I would add “local” before “extreme warming events” to emphasise the grid-point based defined events, as the authors are here referring to the studies where local buy observations are utilized.
- L170: Add “mainly” after “Therefore, we focus”, because the authors do return to the high Arctic definition later on in the paper, as in the trends.
- L292: I would add the temperature unit after the “zero” (mentioned twice on this line)
- L 400: did the authors forget to include “do” prior to “… changes in AR frequency …”?
- L432: shortly remind the readers here what is meant by “concurrent warming events”