Response to Reviewer 3

Dear Dr. Fontana Bach,

I thank you for your valuable comments on the manuscript. My response to the comments and the changes I plan to make in the revised manuscript are detailed below. For clarity, the comments are in blue font, while my response is in black. In some cases, I have included text planned to appear in the revised manuscript in red font.

General comments:

This interesting manuscript provides an in-depth analysis of interannual SWE variability and long-term climate change effects on SWE over northern Europe. The study uses reanalysis data and Regional Climate Models under climate change scenario RCP8.5. The author disentangles the components of SWE variability into the contribution from three components. This provides a clear view on the effects and interplay between warming temperatures and increasing precipitation over northern latitudes. Here, a clear distinction is made between (i) the effect of rising temperature and precipitation due to climate change, showing that temperature clearly dominates the future climate leading to an overall decrease in SWE, and (ii) the fact that in the current climate, warmer years have higher SWE due to different prevailing atmospheric conditions leading to higher precipitation (when still cold enough). Although there is some uncertainty associated to the methods used, the results are robust, novel, and provide a great contribution to scientific knowledge on the effect of climate change on snow. The manuscript is well structured and well written, so I support its publication.

I can only add a few comments to clarify and generate discussion on a couple of matters.

Specific Comments

Line 57: What is the reason that such a low correlation (r>0.32) is significant at 5%level? Very high variability?

The value of correlation required for statistical significance is determined by the sample size. With data for 39 winters available and neglecting interannual autocorrelation, there are 37 degrees for freedom in the calculation of the correlation coefficient. The weakest correlation that is significant at the 5% level is then slightly less than ±0.32 (e.g., https://www.real-statistics.com/statistics-tables/pearsons-correlation-table/), assuming that the distribution of the data is not far from normal.

Lines 59-69: Although I agree that only “the first part of the reasoning is correct” regarding the analogy with the future climate, there is also observation-based research showing that snowfall and snow depth have already been increasing over some parts of Scandinavia and Eurasia (even if the reasons are not entirely clear). , I suggest to extend this paragraph and discuss these references too

https://doi.org/10.2166/nh.2012.109
https://doi.org/10.1029/2018GL079799•
https://doi.org/10.5194/tc-12-227-2018-

Thanks for pointing out these references. I will discuss them in the revised manuscript, but I feel that they are easier to put in context after showing the projections from the EURO-CORDEX simulations. Therefore, I plan to leave this part of the Introduction nearly as is, except for replacing "earlier research suggest" (L64 in the original manuscript) with "climate model projections suggest". The suggested references will be discussed in a new paragraph in the end of Section 6, planned to read approximately as follows. Note that the last sentence reflects one of your later comments!

A caveat in any model-based analysis is that climate changes in the real world may or may not follow the model projections. Interestingly, despite a decrease in winter mean and maximum snow depth in large parts of Europe since the 1950s (Fontrodona Bach et al., 2018), Skaugen et al. (2012) found generally positive trends in winter maximum SWE above the 850 m altitude in southern Norway in the period 1931-2009. On a larger scale, Zhong et al. (2018) analysed observations of winter maximum snow depth in the Former Soviet Union, Mongolia and China, finding an average positive trend of 0.6 cm decade^{-1} from 1966 through 2012. Increases in snow depth dominated especially north of 50°N, extending to milder regions than one would expect based on GCM projections for the future (Räisänen, 2008). Whether such differences reflect a problem in the models or have resulted from multidecadal internal variability in the atmospheric circulation (Deser et al., 2012; Mankin and Diffenbaugh, 2015) is still an open question. If the atmospheric circulation turned out to be more sensitive to increasing greenhouse gas concentrations than current climate models indicate (as tentatively suggested by Scaife and Smith, 2018), some of the present conclusions might need to be modified.

March is chosen as a key month because of its maximum in SWE over most of the area, but March SWE is dependent on P and T of the previous winter months too.I n Figures 1,2,3 March SWE is compared to variability in NDJFM temperature and precipitation. However, in Figures 5, 9 and 12, it is not clear to me if the decomposition of variability into the three components is done only for March or for the entire winter. Perhaps this is clear from the mathematical theory presented, but a clarification and justification of this would be appreciated.

The diagnostic analysis integrates the effect of weather conditions from August until the month of interest (e.g., March), rather than using the data for this month alone. Furthermore, although the NDJFM season is used in some of the figures to provide and overview of the cold season weather conditions, it has no specific role in the calculation. To explain this better in the revised manuscript, I plan to revise the paragraph below Eq. (2) as follows:

Thus, the difference in SWE is decomposed to contributions from the differences in total precipitation (ΔP), snowfall fraction (ΔF) and the snow-on-ground fraction (ΔG), plus a non-linear term that is typically much smaller than the first three right-hand-side terms in Eq. (2). As in Eq. (1), the time integrals in Eq. (2) start from August. The four right-hand-side (rhs) terms in Eq. (2) therefore integrate the effect of weather conditions from August until the month of interest (e.g., March), although the first month that matters in practice is the first month with non-zero mean snowfall. Thus, although the NDJFM season is used for characterizing the winter temperature and precipitation in some of the figures, the diagnostic analysis also uses data outside of this season.

Table 2: Might be my lack of understanding, but I do not know what the values in parentheses mean. What are the "individual terms"? How are they different from the four rhs terms in Eq. 2? It would be helpful to clarify this.

The values in the parentheses are the interannual standard deviation of each term, and its correlation with the actual SWE anomaly. To point out the connection to the earlier equations is an unambiguous way, the table caption will be modified as follows. Note that the earlier Eq. (4) will be Eq. (5) in the revised manuscript.

Table 2. Standard deviation (mm) of detrended March mean SWE anomalies in years 1982-2020 decomposed to its contributions from the four rhs terms in Eq. (2). The values in parentheses give the standard deviations of the individual terms ($s(\Delta SWE_i)$ in Eq. (5)) and their correlation with the SWE anomaly ($r(\Delta SWE_i, SWE)$ in Eq. (5)).

Lines 333-335. Regarding the positive correlation between SWE variability and temperature due to the westerly flow anomalies. Would the RCMs considered here, with boundary conditions from GCMs, reproduce any change in these anomalies which could have a strong effect in the future? Maybe just worth discussing this possibility.

I agree that it is prudent to leave the door open for the possibility that the weak circulation response in the models is incorrect. To do this, I plan to add the following sentence in the very end of Section 6:

If the atmospheric circulation turned out to be more sensitive to increasing greenhouse gas concentrations than current climate models indicate (as tentatively suggested by Scaife and Smith, 2018), some of the present conclusions might need to be modified.

-Lines 354-356: Could this relate to the contrasting response of mean snow fall and extreme snowfall to warming as shown in https://doi.org/10.1038/nature13625 and O'Gorman https://doi.org/10.1007/s00382-015-2587-0 Räisänen

Yes, this may be part of the explanation, together with an overall decrease in the number of snowfall days and increased frequency of melt events. I plan to add a note on this in the second paragraph of Section 7 (the second sentence below):

This suggests that the snow conditions are becoming increasingly irregular, with an increasing number of virtually snow-free winters but a smaller relative decrease in SWE in the most snow-rich winters than in an average winter. Apart from an increasing frequency of midwinter snowmelt events, this likely reflects an increase in relative snowfall variability as the number of days with snowfall decreases but the intensity of the largest snowfall events remains nearly unchanged (O' Gorman, 2014; Räisänen, 2016).

Given the choice of scenario RCP8.5, and the sensitivity of this type of research to crossing or not crossing the freezing threshold (snow or no snow), it would be good to raise a point in the conclusions whether how different might the results under another scenario. Or to call for future work on the analysis of multiple scenarios.

If the greenhouse gas emissions were smaller than those in RCP8.5, all the climate changes, including the rate at which SWE decreases, would mostly likely be smaller. It would also take longer for the changes to emerge clearly from the background of natural variability. On the other hand, the projections for the mid-century period 2020/21-2058/59 are qualitatively similar to those for 2059/60-2097/98, although the radiative forcing is much weaker. This suggests that the basic, qualitative nature of the changes should not be very sensitive to the choice of the scenario, although their magnitude is. Considering this comment, I plan to extend the second last paragraph in the Conclusions section as follows:

… These considerations qualitatively explain both the geographical contrasts in the drivers of the present-day SWE variability and the shift towards increasingly snow-on-ground and snowfall fraction dominated SWE variability in a warmer future climate. Under a scenario with smaller greenhouse gas emissions, this shift as well as the changes in mean SWE would most likely proceed more slowly than the present results for RCP8.5 indicate, and it would take longer for them to rise above the background of natural variability. However, the qualitative similarity between the multi-RCM mean projections for 2059/60-2097/98 and the midway period 2020/21-2058/59 (Figs. 9, 10, 12 and 13) suggests that the basic characteristics of these changes should be largely insensitive to the magnitude of the radiative forcing.

Note that, in response to the comments of Reviewer 1, I will add a new Fig. 8, and the figure numbers thereafter are changed accordingly.

Technical corrections:

Line 57 in caption should be: (c) NDJFM mean precipitation (not temperature)

Will be corrected.

Lines 150-154: Please add also “Equation” to “1” and “2”, to clarify.-

Sorry for being unclear with the notations. “1” and “2” here referred to the subscripts introduced just above Eq. (1), not to the numbers of equations. The planned clarification is as follows:

In this study, the decomposition (2) is applied in two different ways:

1. When studying interannual variations in SWE, X_1 as defined above Eq. (2) represent the mean values for a 39-winter period (1981/82 to 2019/20, 2020/21 to 2058/59 or 2059/60 to 2097/98) and X_2 the values for an individual winter.
2. When studying long-term changes in SWE, X_1 represent the mean values for winters 1981/82 to 2019/20, and X_2 those for either 2020/21 to 2058/59 or 2059/60 to 2097/98.

Line 319: Should be Fig. 10c (not 9c).

Thanks for noticing this. Will be corrected.