Response to Reviewer 2

I thank the reviewer for his / her 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.

Summary

This is an interesting and illustrative study documenting the impacts of different drivers on snow cover variability in northern Europe and evaluating the impact of global warming on these drivers. Specifically, the study elaborates why rising temperatures with increasing precipitation levels do not result in increasing snow depth even in the coldest areas of the study region where milder winters tend to be more snowier than colder winters. The study is in general well written and easy to follow and I have only a few comments as outlined below.

Specific comments:

Part of the cited literature is not included in the reference list, e.g., Lehtonen, 2015 and Räisänen, 2019.

It is a shame to confess that I apparently forgot to check the list of references. In addition to Lehtonen (2015) and Räisänen (2019), van Vuuren et al. (2011) was also missing. The details of these three references are as follows:

Lehtonen, I., Four consecutive snow-rich winters in Southern Finland: 2009/2010–2012/2013. Weather, 70, 3-8, doi: 10.1002/wea.2360, 2015.

Räisänen, J.: Effect of atmospheric circulation on recent temperature changes in Finland, Clim. Dyn., 53, 5675-5687, doi: 10.1007/s00382-019-04890-2, 2019.

van Vuuren, D. P., Edmonds, J., Kainuma, M., Riahi, K., Thomson, A., Hibbard, K., Hurtt, G. C., Kram, T., Krey, V., Lamarque, J.-F., Masui, T., Meinshausen, M., Nakicenovic, N., Smith, S. J., and Rose, S. K.: The representative concentration pathways: an overview. Climatic Change, 109, 5-31, doi:10.1007/s10584-011-0148-z, 2011.

Lines 87-93: I agree with the editor that it would be best to introduce whether ERA5-Land assimilates in-situ measurements and/or remotely sensed data or not in the first paragraph of this section.

As was mentioned in the original manuscript (L91), ERA5-Land uses no data assimilation (although, of course, the “parent” ERA5 renalysis does, and this affects the meteorological forcing seen by the H-Tessel land surface model used for generating ERA5-Land). However, looking more closely at the documentation of ERA5, I found that I had been unclear about one detail:

although ERA5 assimilates observations of surface air temperature, it only assimilates precipitation measurements (to some extent) in North America, and not in Europe. This will be pointed out in the revised manuscript.

Lines 110 and 443: The link to the FMI website can be given in English as follows: https://en.ilmatieteenlaitos.fi/download-observations

Thanks. This address will be changed in both places.

Line 143: Is the non-linear term included into the equation just to explain residual SWE variations not explained by the first three terms?

Yes, the inclusion of the non-linear term makes the equation exact (the derivation is simple, although not shown in the manuscript). I therefore retain this term, although it has little practical significance.

Fig. 3: Just as a side note, perhaps the negative temperature bias in ERA5-Land in Helsinki is at least partly due to urban heat island effect whereas in Sodankylä the positive bias is likely most significant in cold weather situations with marked temperature inversion. It even seems that the bias is larger in cold than mild winters, supporting this latter hypothesis.

Both of these speculations are probably correct. I plan to include a brief mention on the urban heat island effect. However, it is probably not necessary to discuss the inversions in Sodankylä, because this would require a longer explanation which could distract the flow of the text – particularly recalling that the cold winter climate in Sodankylä makes SWE generally less sensitive to temperature than precipitation.

Fig. 7: I am just wondering whether it would be more interesting to show in the second panel the ratio of standard deviation of SWE to the mean SWE, i.e., SD(SWE)/Mean(SWE)? I am not saying it would be a better option but left the choice for the author, but this would highlight more clearly the point raised on lines 233-234 about the areas with higher variability. Perhaps it would just mirror the left panel.

After considering this comment, I decided to retain Fig. 7 as is, to keep its interpretation as simple as possible. However, I have prepared separate maps for the coefficient of variation, which I plan to include as an appendix (see below):

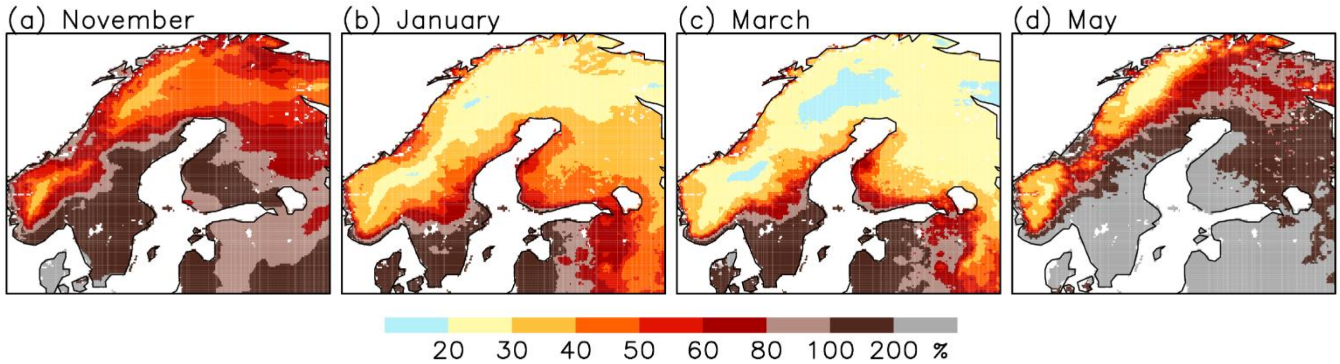

Figure A1. Coefficient of variation of monthly mean SWE in ERA5-Land in (a) November, (b) January, (c) March and (d) May.

Fig. 8: I’ll suggest a small change in the caption. It would be clearer to state that "Middle: the changes from 1981/82–2019/20 to 2020/21–2058/59: : :" and "Right: as middle, but from 1981/82–2019/20 to 2059/60–2097/98"

Suggestion accepted.

Fig. 10: Interestingly, the largest precipitation increase in relative terms is projected to take place in the eastern side of the Scandinavian mountains (Fig. 10b), although increase in the easterlies is not usually projected by climate models in this region. Comparison of Fig 10b with Figs 10d and 10e even suggests a shift towards dynamically colder weather types as the projected increase in precipitation is smallest (virtually negligible) along the Norwegian coast, where the positive coupling between mean temperature and precipitation is the strongest, while the only areas in Sweden where precipitation levels tend to be even slightly higher in cold than warm years are among the areas with largest projected increase in precipitation.

I also find this distribution of precipitation changes somewhat surprising, although it is qualitatively similar to that found by Räisänen and Eklund (2012, Climate Dynamics, 38, 2575-2591) for the earlier ENSEMBLES RCM simulations. I tried to study its origin by looking at the time mean sea level pressure changes in the EURO-CORDEX RCMs, but found these changes to be too small to provide any obvious explanation. This is thus clearly an issue for further research. The planned text discussing this in the revised manuscript is as follows:

The EURO-CORDEX RCMs simulate, on the average, a NDJFM mean warming of ca. 3-5°C from 1981/82-2019/20 to 2059/60-2097/98, with a general increase from southwest to northeast (Fig. 11a). The change in precipitation varies from slight local decreases in western and northern Norway to increases of up to 25%, with a relatively sharp northwest-to-southeast contrast across the Scandinavian mountains (Fig. 11b). This contrast is qualitatively similar to that found by Räisänen and Eklund (2012), but its connection to the atmospheric circulation in the EURO-CORDEX RCMs would require further investigation. The multi-RCM mean changes in the NDJFM mean sea level pressure in northern Europe are small (from 0 to +1 hPa), implying only very modest changes in the average lower tropospheric winds (not shown).

Fig. 11: It seems like the model data has a warm bias at least in Sodankylä, as the interannual standard deviation of SWE peaks already in April indicating an earlier melt season compared to ERA5-Land showing the peak in May. Perhaps it is thus unlikely that even in the case of RCP8.5, the peak melt season would shift to as early as March by the end of century.

The early snowmelt during the baseline period naturally affects the quantitative interpretation of the future projections. This will be pointed out explicitly in the revised manuscript, as shown below. Note that I plan to add a new Figure 8 in the article (see the response to Reviewer 1), and the old Fig. 8 will therefore become Fig. 9 and Fig. 11 will become Fig. 12.

Note, though, that the standard deviation of SWE in Sodankylä in years 1982-2020 reaches its maximum earlier in the RCMs than in ERA5-Land (bottom left of Fig. 12 vs. Fig. 6b), just as the mean SWE does (bottom left of Fig. 9 vs. Fig. 4l). This bias naturally affects the quantitative interpretation of the model projections.

Lines 418-423: Nice that the sources of uncertainty are recognized and acknowledged by the author. This is not obvious in all the studies.

Thanks for this fcomment. In fact, considering the comments of the other reviewers, I decided to expand the discussion of uncertainties in the Conclusions section, as follows.

This study relied on the ERA5-Land reanalysis in diagnosing the interannual SWE variability. The use of a reanalysis instead of direct observations was dictated by the lack of observations for the snowfall and snow-on-ground fractions (in-situ observations of SWE are also limited in number). The good agreement on snow depth between ERA5-Land and station observations (Fig. 3) is encouraging, suggesting that the dynamics of SWE variability may also be well represented. Still, the model-dependence of reanalysis products might affect some of the current results. For example, a good simulation of SWE might hide compensating errors in the snowfall fraction and snow-on-ground fraction, which are both difficult to verify but are potentially sensitive to the simulation of precipitation microphysics and the description of snowmelt, respectively. Unfortunately, few if any comparable data sets are currently available, since most reanalyses have coarser resolution than ERA5-Land and/or have artificial sources or sinks of snow due to the assimilation of snow observations (as, for example, in the parent ERA5 reanalysis). Regarding the simulation of the snow-on-ground fraction, off-line comparison of land surface models represents one way forward (Essery et al., 2020).

Lines 433-439: I think this is a rather expected conclusion. It would be surprising if a winter with so extremely extraordinary circulation patterns than 2019/20 would be an analogous winter of the future as the winters dynamically analogous to 2019/20 will remain rare in the future, though potentially turning slightly more common than in the past.

I agree that this conclusion is not unsurprising, but I still hope and believe that it was worthwhile to demonstrate why interannual variability is not a good analogy for the long-term climate change effect on snow conditions.