The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-343-RC2, 2021

The Cryosphere
Discussions

Interactive comment on "Snow conditions in northern Europe: the dynamics of interannual variability versus projected long-term change" *by* Jouni Räisänen

Anonymous Referee #2

Received and published: 2 February 2021

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:

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Discussion paper

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

The two figures in the Introduction section fit very well in the section.

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.

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

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

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.

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.

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"

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.

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.

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

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.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-343, 2020.