The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-343-RC3, 2021

The Cryosphere Discussions
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EGU

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

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Received and published: 8 February 2021

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 pre-

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Discussion paper

cipitation 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?

- 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

- 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. In 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.

- 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.

- 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.

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

- 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.

Technical corrections

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

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

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

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