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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 #1

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Summary;

The author discusses the mechanisms behind interannual variability in snow depth/SWE in Northern Europe, and how these mechanisms differ from that of the projected long-term climate change. Analysis is performed on the ERA5-land data set and regional climate models from EURO-CORDEX, by an earlier published method de-composing the contributions from change in total precipitation, snowfall fraction and snow-on-ground fraction (melting).

The paper is well written, easy to follow and fit well in The Cryosphere. The work is relevant for everyone that wants to understand the mechanisms behind changes in

snow on ground in more detail and explains why "warm winters" of today's climate can't be used as an analogy to future projected climate change with respect to snow cover. In general I like the paper very much, but have some comments and questions which hopefully will improve and clarify certain aspects.

General comments;

The scientific problem investigated is in itself interesting and line 59-60 is a start to put the work in a broader context and meaning. However, in the introduction I miss some further elaboration on why this work is important for a wider audience. Related to this, in the conclusions, is it possible to say something on the consequences of the findings in the study. For example (my understanding) does it mean that the accuracy of the snow schemes (i.e. melting processes) is important to get correct to simulate future changes in snow amounts in northern Europe?

The study refers to some earlier works, but not very many. However a google search shows many results on scientific papers on snow cover in Europe and also with examples of using data from, e.g. Sodankyla. This reviewer doesn't know the details of these papers, but imagine that at least some of them might be relevant to mention in the context of this work. For example the recent Cryosphere study looks relevant(?)

Essery, R., Kim, H., Wang, L., Bartlett, P., Boone, A., Brutel-Vuilmet, C., Burke, E., Cuntz, M., Decharme, B., Dutra, E., Fang, X., Gusev, Y., Hagemann, S., Haverd, V., Kontu, A., Krinner, G., Lafaysse, M., Lejeune, Y., Marke, T., Marks, D., Marty, C., Menard, C. B., Nasonova, O., Nitta, T., Pomeroy, J., Schädler, G., Semenov, V., Smirnova, T., Swenson, S., Turkov, D., Wever, N., and Yuan, H.: Snow cover duration trends observed at sites and predicted by multiple models, The Cryosphere, 14, 4687–4698, https://doi.org/10.5194/tc-14-4687-2020, 2020.

Even if it won't change the results from this study it would be nice to tie the present work more together with a broader part of earlier work.

The main work in this study relies heavily on the ERA5-Land data set. Some verification of ERA5-Land is also included at Sodankyla and Helsinki. The results of the verification in itself are encouraging. However, only comparing ERA5-Land with observations at two locations when discussion the results also in other terrain types, e.g. Scandinavian mountains are a bit scarce. I can imagine that the errors might be larger at other locations. If other studies of the quality of ERA5-land for northern Europe exist they could be referred to. I also miss some verification of the precipitation phase in ERA5-Land, it should be possible to compare from observations, either by manual observations (if available) or by using some temperature thresholds based on the observations. Finally for the verification part, is it possible and meaningful to estimate any of the decomposed contributions to snow variability from observations and compare them with ERA5-Land?

While the presented analysis relies on mean temperatures, how will the local temperature variability impact, e.g. in some areas there might be large variability with several "above zero degree" periods while in other areas it will be less variability and maybe constant temperatures under zero with the same mean temperature? I'm not suggesting any new analysis, but I'm curious about if you have any opinions on this subject? Can this partly explain why the correlation of Fig2a doesn't follow the mean temperatures?

Specific/minor comments;

Line 26: Since the example is one single winter (2019/20) it is not an example of large interannual variations in my view. Is it better to write something like: "An example of a particular anomalous winter was the winter 2019/20 . . .."?

Line 28-29: I suggest to add some details about the regional contrasts in the text, not only refer to Fig 1c. That would improve the flow in the reading.

Line 30-31: Add "(indicated by stippling in Fig 1c)" at the end of the sentence? I used some time to wonder how you knew that this was record low.

Line 42: Can the word "interannual" be skipped in this sentence?

Line 68-69: Since SWE and snow depth are not equal they are not necessarily straightforward to compare, e.g. a change in snow depth can also come from changes in snow density. Is this important or discussed anywhere in the references?

Line 75: In my first read I wondered what the exact meaning of "the fraction of accumulated snowfall that remains on the ground" was. It is explained better later, but can something to clarify what it is be added already here (e.g. "the partition not melting")?

Line 102: Without having detailed knowledge about the local terrain/weather around Helsinki and Sodankyla I wonder if at least the coast line near Helsinki introduces some gradients in temperature? But as it is commented, since both Helsinki and Sodankyla observations have a long observation track and distribute observations via GTS they are probably assimilated in ERA5. I think that in combination with the fact that you compare monthly mean and not hourly values ensure the very high correspondence with observations? I guess the correspondence with observations are reduced somewhat if hourly values are compared.

Line 103-106: I agree with the reasoning and think that it is actually quite difficult to say if the true bias of ERA5 precipitation is positive or negative. Can you estimate how large, in percent, the apparent bias is? That can make it easier to judge (if you also know the gauge equipment characteristics and wind climate). I don't think it is necessary to be very quantitative, but it could strengthen your qualitative statement.

Figure 3: I think you can argue that the temperature bias is smaller during precipitation events. In particular the positive bias in Sodankyla probably arises from stably stratified situations with little precipitation.

Equation 2: It would be nice with some more details on how the different terms are calculated from ERA5-Land and the RCMs. For example, for the snowfall, do you use snowfall from ERA5-land (?) or do you use some temperature thresholds on the

total precipitation to decide precipitation phase? Are snowfall available from all RCMs? Even more, I'm curious on how you calculate the G-term in practise. Can you give some more details?

Line 150-151: Isn't "1" applied on single winters and then the average is taken over all these winters? Could the word "decomposed" be added in front of "values for an individual winter"?

Line 152-153: Again isn't "1" applied on single winters and then averaged? Or do I misunderstand?

Equation 3 & 4: I don't understand these. I think they need some more explanations. That would help me to understand e.g. Line 206-207 also.

Figure 7: What does the "C" in "SDC" stand for?

Line 261: add "change" after "SWE"?

Figure 10. The discussion of Fig 10 is not clear to me. I don't fully understand what the message is. Can this be made clearer?

Line 426: "colder areas" Is it possible based on this study to quantify/define what "colder areas" are? Wouldn't that be of interest for many, when a local climate enters a new regime where variations in winter precipitation is no longer the main driver?

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